Philosophy of Science

VOL. 1 April, 1934 NO. 2

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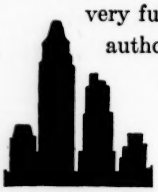

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THE
WILLIAMS & WILKINS COMPANY
Publishers of Scientific Books and Periodicals
BALTIMORE, U. S. A.

Philosophy of Science

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Meaning and Scientific Status of Causality

BY

HENRY MARGENAU

THE disagreement with regard to the validity of the principle of causality, existing to-day among scientists, has its roots in the diversity of definitions of the principle itself rather than in a problematic scientific situation. As far as the formulation of quantum theory is complete its bearing upon philosophical questions can be fixed with precision provided the questions are phrased intelligibly. But a question is intelligible from a scientific point of view only if it satisfies two conditions: (1) the meaning of its terms must be fixed; (2) it must be in accord with the conventions of the science to which the question is put.

The necessity of the first requirement is at once evident; if it were not satisfied the question would have several correct but self-conflicting answers, such as those to which the undisciplined discussion of philosophical problems usually leads. The second requirement, however, reflects a particular weakness of philosophy. Within the domain of the latter, words have retained a variability of meaning which, to be sure, makes for beauty and flexibility of expression, but impairs precision of speech. So per-

sistent was the tendency toward figurative flourish that even in cases where science had standardized the meaning of a term in a very systematic and useful way philosophers continued to use the term with its former diffuseness. When accused of this procedure they said they were speaking non-technically and deplored politely the scientist's manner of degrading terms into technical ones. Indeed the difference between philosophy and the specialized sciences began when the latter caused their concepts to crystallize and agreed to name them universally and with care. Even now the distinction between science and philosophy is best described in saying that science proceeds by making the acceptance of its terms obligatory for all its pursuants, while philosophy allows its advocates to coin largely their own phraseology. This individualism frequently causes confusion and indefiniteness of philosophical attitude. To illustrate: whether causality is a category or not, is chiefly a matter of definition and nomenclature and may be answered correctly by yes and no, in fact the question may be meaningless; proponents for each of these three answers are to be found among modern philosophers. However, the question: is mercury an element has but one correct answer. Similarly, the term energy which has a perfectly definite scientific meaning is constantly used in phrases such as "mental energy" which signifies nothing unless ignorance of the laws of physics on the part of the speaker. Science has avoided such ambiguities. Hence we can effectively guard against them by using technical lingo wherever it is possible. In discussing causality it is necessary to formulate the problem in terms of physics as far as they are available.

Considerations like these are common with all scientists and many philosophers to-day; they constitute no unsympathetic critique of philosophy, or of the methods employed in philosophical investigations. For the solution of numerous problems the methods of science, which, speaking figuratively, is nothing but the crystallized part of philosophy, are not, or at least not yet, at hand. It is also true that the philosophical terminology is more strongly subjected to popular misuse than scientific language, and therefore prevented from being standardized. None of these

arguments is sufficient, however, to justify the use of vague philophical phrases where definite scientific terms present themselves.

Kant's formulations of the causality principle are to be rejected mainly for these reasons. When he says in the first edition of his "Critique of Pure Reason:" "Alles, was geschieht (anhebt zu sein), setzt etwas voraus, worauf es nach einer Regel folgt," this statement is not scientifically clear. "Regel" is entirely undefined; it is possible to find a rule for everything that is susceptible of description. On the other hand, if the word is to be interpreted as a means of knowing subsequent events in advance, there arises the difficulty of who is to know and employ the rule, together with all the other inconsistencies which will be encountered shortly in connection with similar formulations. Another possibility of interpretation places the emphasis upon the first part of the sentence and neglects the last as an inessential explanatory phrase. But then, if "voraussetzen" is taken in its temporal sense, Kant's statement amounts to nothing more than the assertion that the universe has no beginning in time, which is plainly not identical with the causality postulate.

Kant's modified formulation, as it appeared in the second edition of the same book, reads: "Alle Veränderungen geschehen nach dem Gesetz der Verknüpfung von Ursache und Wirkung," and is subject to the same criticism. Physics knows of no such law; as a matter of fact there is no plausible way of defining cause and effect. No laws of physics, if properly stated, involve reference to either of these concepts, and if the distinction of cause and effect is artificially impressed upon the phenomena which these laws regulate, then the laws do not even allow us to differentiate between the two. This fact follows at once from the well known property of reversibility possessed by all natural laws. Newton's law of gravitation, for instance, sets up a relation between an observation on the rate of change of the radial velocity between two masses on the one hand, and the distance between them on the other. But it contains no criterion to determine the causal status of these observations. There is no law of connection between cause and effect known to science; moreover, these concepts are foreign to physical analysis. Nor

is it of any avail to inject them externally, for the meaning usually conveyed by the words in question is expressed more adequately and precisely by technical terms like boundary condition, initial and final state.

Most of the difficulties discussed so far are avoided in Laplace's statement of what he and many later scientists consider to be the essence of causality. He postulates the existence of a universal formula according to which all happenings take place, and expresses this state of affairs as follows (Théorie analytique des probabilités): "An intelligence knowing, at a given instant of time, all forces acting in nature, as well as the momentary positions of all things of which the universe consists, would be able to comprehend the motions of the largest bodies of the world and those of the smallest atoms in one single formula, provided it were sufficiently powerful to subject all data to analysis; to it, nothing would be uncertain, both future and past would be present before its eyes." This is certainly an intelligible proposition; it is excellent in its clarity and precision. All of its terms are well defined; the word force is to be understood in its accurate physical sense as the product of mass and acceleration, and "knowledge of a force" means knowledge of the differential equation which relates this product to a function of position, this function being also known. This spirit of Laplace's propositon pervades all of classical physics and has been eminently fruitful in the development of that science. Is the proposition true? In answering this question we shall find reason for abandoning this particular formulation of the causality principle.

Of course Laplace's statement is true. Imagine if possible a perfectly arbitrary universe with a god agitating it according to his ever-changing desires. Although we are still searching for a suitable definition of causality, it seems clear that this would constitute the model of a non-causal world. If we further suppose the happenings in this chaotic universe to be discernible and describable, then it must be possible also to represent them by means of equations. These equations will not necessarily contain analytic functions only, nor will they be differentiable. However, if things take place with reasonable smoothness and not too sud-

denly, if "natura non facit saltus," the functions will possess the property of differentiability. It is then clear that differentiation will, in general, simplify the equations, for it will cause additive constants to diasappear. An intelligence powerful enough to know all these differential equations together with the values of coordinates and derivatives at a given time, and able to solve them, would have a complete survey of all events, future and past. In Laplace's world this survey must be possible on the basis of a knowledge of all forces, i.e. differential equations of the second order. This, in itself, constitutes no further restriction upon our arbitrary universe, since there is nothing to prevent us from differentiating the equations twice; but it expresses a preference which should manifest itself in a universe satisfying this particular causality postulate. In such a world laws should take on an especially simple form if they are stated as second order differential equations. Thus it is seen that Laplace's postulate is not a stringent one, it is true for almost any imaginable universe. It imposes nothing upon a world which by itself runs smoothly, and is certainly a valid approximation to the course of events in the arbitrary, non-causal universe. In that sense the statement in question is true, but it does not seem to characterize causality. Whether or not nature is conveniently describable in terms of second order differential equations is an entirely different issue and must probably be affirmed—although there are cases of physical analysis where description by equations of higher order is customary.

The fact that the postulation of a universal formula, as phrased by Laplace, involves the hypothetical existence of an omniscient demon has been considered unsatisfactory by numerous investigators. An excellent critique of this point is to be found in Ph. Frank's recent book: Das Kausalgesetz und seine Grenzen. The appeal to a higher intelligence is certainly unscientific and to be avoided if possible. But it is not intrinsically bad if it merely serves to clarify the proposition. The question is: does Laplace's supposition of a superior intelligence constitute an essential point of his statement? Evidently not, for if the reference to this intelligence were omitted the assertion, weak as it may be,

that nature be conveniently describable in terms of second order differential equations would still remain. But this can be progressively tested by experience and admits of definite verification. Hence it is not legitimate to say that the demon does not exist or is impossible and therefore reject the proposition. A real criticism should attack its meat and not its form. It may well be observed in this connection that a very common argument against causality fails for the same reason. This argument appeals to the uncertainty principle which does not permit all the simultaneous data necessary to integrate the universal formula to be known. Hence, it is concluded, the causality principle can not be valid. Here, too, it is forgotten that the criticism is directed against an inessential point in Laplace's formulation which, as we have seen, fails to express the characteristics of a causal universe anyway.

The differential equations governing the processes in the arbitrary, lawless world which has been imagined will in general involve the time explicitly. Consequently the forces (which are always to be defined in terms of accelerations, not popularly as "pushes and pulls") will change with time in an essential manner, and not only through the coördinates on which they depend. In order to predict the future Laplace's demon would have to know not only the instantaneous values of all forces, he would require their complete form as functions of the space coördinates and of time. An entirely different situation arises if we postulate that the forces be functions of space coördinates and possibly their time derivatives only. This would constitute a very definite limitation upon natural processes, a limitation indeed which the world agitated by a god would fail to exhibit. For now the forces have the same instantaneous values whenever the coördinates and their derivatives assume a given set of values; this endows nature with a regularity which, it appears to us, is very nearly what causality is meant to convey. But this important feature hardly follows from Laplace's formulation and may, at any rate, be expressed more directly as we shall see presently.

Before continuing this trend of thought it seems necessary to deal with a definition of causality which is most widely accepted at present. The argument runs: introducing a superior intelli-

gence is not permissible because it is man who makes his science and it must be he who is to judge whether his world is causal or not. A universal formula without an individual knowing it is a vague phrase. Why not modify the proposition last considered by substituting man in place of the demon? The causality principle is then valid if it is possible for the scientist, on the basis of known laws, to reconstruct the past and to project the future when the present state of the world is completely known. We shall term this conception of causality, in which a clearly anthropomorphic attitude combines with utilitarian considerations, the positivistic one for want of a better name. It is quite in harmony with the modern trend of eliminating things that have merely logical status but no concrete meaning in terms of physical operations. Nevertheless it pays to examine it closely.

First it is defective from the point of view of intelligibility. The term "possible for the scientists," or, if it is preferred, "possible for the human mind" is objectionable. If it conveys its popular connotation, who is to decide the capabilities of the scientist or the human mind? Even though a present physical theory denies the possibility of knowing all data upon which a detailed analysis of the future depends, this possibility may be restored by later developments. It is true that this fault can be remedied by modifying the positivistic formulation of the causality principle and stating: causality exists if no confirmed physical theory contradicts the acquisition of data by which a determination of future and past events can be made. However, this is very specific and has never been proposed, nor is it exempt from the criticism that follows. In line with the present argument we also note the occurrence of words like "law" and "theory" which, though they figure prominently in physical discussions, still lack universal definitions and are very far from being technical terms. Direct statements about nature are always clearer and less involved than terms like these which refer to our reasoning about nature, and are therefore better suited to define causality. If such considerations seem pedantic one must answer that their neglect has caused more difficulties than has the actual solution of philosophical problems.

Yet they do not touch the principal weakness of the positivistic causality formulation. We propose to show that the latter may be satisfied by a non-causal universe. The point is simply this: causality has nothing to do with the question whether future events may be known in advance, its prerequisite is not that the scientist turn prophet. Suppose that the god who agitates his universe according to his inscrutable desires and without restrictions by law or order should give the scientist exact forewarnings of his actions, so that the latter is able to prophesy with accuracy. Would this make his playland a causal universe? Of course there is no rigid answer since we are still searching for a suitable definition of causality. But if we interpret correctly the universal implication of this concept regardless of its various formulations we feel that one must answer: no. It is commonly conceived, for instance, that the occurrence of miracles contradicts causality; in fact disbelief in miracles is usually justified by the causal constitution of the world. The circumstance that many miracles have allegedly been predicted is hardly sufficient to restore a sceptic's belief in them, and hence to reconcile their occurrence with the causality postulate. We conclude: the positivistic formulation with its main emphasis upon human ability to know in advance does not express the nucleus of what is understood by causality. Besides, it is anthropomorphic and reflects distinctly the present utilitarian color of our science.

Let us now put an end to criticism and select a definition that will satisfy the outlined requirements more widely and state the central part of the concept in question. It will be granted that the crucial feature which makes the arbitrary world non-causal is the irregularity arising from the whims of the god, whether this irregularity may be known or not. To be more specific it is the fact that in a non-causal world the force between two electric charges varies, say, as the inverse second power of the distance to-day, but possibly as the inverse tenth power to-morrow. Or even, while they attract each other to-day, they may repel each other to-morrow. We feel that causality is violated when a given state A is not always followed by the same state B. A definition of the word "state" is certainly necessary, but we may

reserve it until later. At present the customary intuitive meaning will suffice. This property by which any given state has associated with it a unique consequent state has previously[1] been called "consistency of nature." To avoid circumlocutions we shall continue to use this term. In more adequate phraseology, and without the unsatisfactory reference to states, consistency of nature may be characterized by saying: As a result of the constitution of nature, the differential equations by means of which it is described do not contain explicit functions of the time. This statement is less definite than it seems because it does not contain directions as to the choice of variables appearing in the equations, a vagueness which is the counterpart of the indefinite meaning of "state." The existence of variables must of course be supposed since they form the condition under which description of nature is possible at all. But then, if nature is consistent in this sense, the integrated equations will not depend on time in an absolute manner; more specifically if $x = f(t)$ is a solution, then $x = f(t - t_0)$ is another, so that the motion in question has an arbitrary beginning in time which becomes fixed only if accessory conditions are known. Furthermore, to use a previous example, the exponent in Coulomb's law of attraction will be invariable in time; if it is -2 to-day it will be -2 forever. The same should hold for all parameters appearing in the differential equations of physics if nature is consistent or causal.

It is to be remembered, however, that the occurrence of equations which do not satisfy this requirement, is not at once a proof against causality. In fact we often encounter such equations when a problem is not completely analyzed, for example in the case of forced oscillations. But here, as well as in all similar cases, the explicit time dependence could be eliminated by including in the analysis those parts of the system which produce the varying force, i.e. by "closing" the system. "Impressed forces" always indicate that the physical system in question is an open one. In fact a closed system is simply one to which causal analysis can be applied, that is one which can be described by differential equations not containing the time explicitly. Another point of im-

[1] H. Margenau, The Monist, Jan. 1932.

portance is this: An equation which originally satisfied the causal requirement will, after a single integration, no longer do so. We express this state of affairs by saying that we now have an "equation of motion" and not a "law." (Incidentally it would be very desirable to standardize this particular meaning of the word law in natural science.) Nevertheless this introduces an uncertainty into our formulation of the causal principle, but one which cannot be avoided. It will occupy us when we discuss the analytic character of causality, its property as a non-tautological proposition.

Consistency, the central issue of the causality postulate, banishes absolute time from the description of nature (equations of motion) by eliminating time explicitly from its *essential* representation (differential equations, laws). It is the only formulation which does so. The elimination is necessary because absolute time has no physical meaning. Another advantage is the simplicity of the consistency formula, which nevertheless involves everything implied in the usual conceptions of causality, such as the existence of unique laws, strict determinism as far as it is a concomitant of causality. (It does not imply, however, that future events be actually known or knowable by an individual.) Theorems of conservation (energy, momentum), generally felt to be in some way connected with the causality principle but unaffected by its customary formulations, follow at once as analytical consequences from the fact that the differential equations do not involve the time explicitly.

The principle of consistency, and hence of causality, has no meaning if it can be applied only to the universe as a whole. For in that case the number of describing variables would probably be infinite and the description in terms of differential equations loses its sense. Using the more intuitive definition according to which a state A is always followed by the same state B the failure is evident when we realize that state A may occur only once. Causality is then an empty phrase unless the universe is periodic. But the same would be true of all the laws of physics, in fact of physics as a science, if its statements were inapplicable to small domains of nature. Hence the condition that causality shall have meaning is the same as that for the existence of science.

The philosopher who argues that the consistency definition be void forgets that his argument nullifies science as a whole. As a matter of experience the universe *is separable* into smaller systems to which differential equations can be applied, and if these equations are of the type here postulated then nature is causal.

An observation of this kind carries little weight with those who feel too keenly that the processes in the universe are separable only to a rough approximation. One is entitled to discuss this attitude in earnest only if he is willing to accept all of its consequences which include the proposition that science is an illusion. But we note in the first place that it is by no means necessary for an arbitrary nature to be *roughly* or *approximately* separable into systems whose fates are independent; to say that it is imposes a definite restriction even if the separation is not completely possible. But let us consider more exactly how it is performed. Measurements on the force of attraction between two electric charges will not in general verify Coulomb's law. We observe that the force depends in some peculiar way upon the position of external charges, which suggests to us that the measured effects are not entirely due to the system in question, namely the two test charges. This is expressed by stating that the system is not completely separated from its surroundings. Next we remove our system farther and farther from surrounding bodies and notice a gradual improvement in the consistency of the measurements. Now the situation would be very simple and satisfactory if progressive isolation produced better and better agreement between the observations. For then the condition of complete isolation, i.e. a closed system, could be defined by a simple limiting process much in the same manner as limits of functions are defined in mathematics. However, the situation is here of greater complexity, though still manageable. The agreement is improved only up to a certain point, and then further isolation fails to make itself felt. We have reached the limit of precision of our measurements. This limit of precision is a very definite thing which scientists have always considered very carefully. Quantum mechanics emphasizes it greatly and even renders its value in some instances calculable by means of the uncertainty relation.

Nevertheless there is a well known method of dealing with the ever present divergence of observations: the theory of errors allows us to compute the most probable value of a measurement from any group of observations. It is the limit of this most probable value upon which we base the derivation of Coulomb's law, and not the limit to which actual observations tend. The latter does not exist, but the former does. Hence it is possible to define a state of separation, not by actual physical operations but by blending experimentation with reasoning. We wish to emphasize in this connection that physical concepts need not—and can not—be defined solely in terms of experimental operations or observations; it is both customary and proper in scientific investigations to characterize an abstract state of affairs by its logical consequences if they are more simply expressible, whether they are observable or not. For example, when we state the second law of thermodynamics in the form: the entropy of the universe increases, then there is no way of testing the law as such; its evidence arises from considerations formally similar to those which have convinced us of the existence of closed systems to which differential equations are applicable.

By its definition, a closed or independent physical system is a causal one, because we call it closed when the laws governing its behavior do not involve the time. But strictly speaking only closed systems are accessible to physical analysis. Thus it would seem that physics can never inform us of a failure of the causality principle. This brings us to a point of importance: Is the causal principle a tautology? Here we are forced to make a large concession; an unbiased investigation must not fail to recognize its character as an analytic proposition. Kant, who thought of it as an a priori synthetic judgment, did not formulate it in a way in which its analytic character became apparent. It is certainly true that whenever a physical system does not appear to be closed, that is when the differential equations describing it contain the time explicitly (if it does not behave consistently), we conclude that the variables determining the state in question are not completely known. We then look immediately for hidden properties whose variation may have produced the inconsistencies,

and whose inclusion in the analysis would eliminate them; moreover if we do not find any we invent them. This procedure is possible because in the consistency formulation of causality the term "state" is undefined. The corresponding indefiniteness in the more abstract formulation lies in the absence of specifications as to the number of variables entering into the differential equations on the one hand, and of the order of the equations on the other. If the number of variables is increased indefinitely, or if an indefinite number of differentiations is permitted, time dependence can be ultimately eliminated no matter how complicated the processes of nature, provided only that the equations of motion can be differentiated a sufficient number of times. But this requires no more than a certain smoothness and continuity which nature certainly satisfies.—On the whole it seems, then, that the causality postulate reduces to a definition of what is meant by "state." It is an agreement to consider those quantities as composing the state of a system which enter into a time free differential equation describing its behavior.

This line of reasoning leads at once to the inevitable question: Why retain the proposition if it is merely tautological? Tautologies, as everybody knows, add nothing to the knowledge of nature; they expose a property already included in the term they are to explain; their careless use often produces vicious circles. All this is true in a sense; but to suppose that tautologies are always useless and to be avoided is a very common fallacy. Every definition is a tautology except the first time it is stated. To the writer the word tautology conveys something more objectionable than the gool old "analytic judgment." Really the two are synonymous, and one must free himself from any intuitive bias that may cling to the former term. The principle of conservation of energy is a tautology in the proper sense of the word, yet nobody doubts its fruitfulness, and it is even customary to speak of its validity as though it were an actual proposition about the world. The point in question is this: The general definition of the principle contains no restriction as to the number of different kinds of energy which may be transformed into one another and whose sum is constant. If this number became indefinitely great as a conse-

quence of invention whenever a new type was needed the principle would hardly be applicable, it would merely define energies. As a matter of fact, however, nature permits us to get along with very few different types of energy, and its description in terms of energies is exceedingly useful and convenient. Therefore, while the energy principle in its logical formulation is a tautology which amounts to a definition of energies, the *analysis of nature in terms of this definition is advantageous*. Precisely the same is true with regard to causality. Its logical formulation is inevitably tautological and leads to a definition of physical "states," or a selection of variables in the differential equations. But this selection is useful and applicable, so that the causality principle, though tautological in its abstract form, does amount to a statement about nature. Moreover it makes sense to say that nature is not causal, for this would be true if, in an attempt to describe nature in accordance with our definition of consistency, the resulting differential equations were found to contain very many variables or to be of very high order. One might, of course, arbitrarily restrict the latter and permit only equations of the second order, but we feel that this would do violence to the common conception of causality.

We have seen that the tautological character of the consistency postulate is no particular fault. Indeed the postulate may be transformed into a synthetic statement if this be desired, but somewhat at the expense of its precision; it might be phrased: Nature is so constituted that its description in terms of differential equations which do not involve the time explicitly (such description is admittedly possible!) is *convenient*.—We merely state that Laplace's causality formulation is also tautological, while the one identifying causality of nature with human power to prophesy is not. But it stands to reason that the latter modification, missing, as it does, the central point of the casuality concept, pays too large a price for its non-tautological form.

In formulating the principle of causality we have almost solved the question as to its validity. Classical physics was based upon it and therefore presents no argument against it. The influence of quantum mechanics upon its status has been considered in

detail in two previous publications,[2] whose results may be stated briefly as follows: Classical analysis had come face to face with experiences which its usual methods failed to describe; in fact the treatment of certain problems threatened to become non-causal. At this very instant quantum mechanics achieved a revolutionary feat of great importance: it redefined the concept of physical states in a more abstract manner (in terms of mathematical functions satisfying certain requirements) and *thereby restored the causal character of physical analysis*. It was the conviction that the causality principle must be retained which inspired quantum mechanics although some of its creators have not been altogether conscious of this fact. The impression that quantum mechanics violates the principle now arises whenever the older classical conception of states (positions and velocities of the component parts of a system) is carelessly carried over into the new field of description in which it has no meaning. The uncertainty principle forbids ultimate extrapolation to the quantities defining a state in the classical sense, but it does not prevent an ultimate extrapolation to ψ-functions. To be quite impartial one should add that the trustworthiness of quantum mechanics even in questions of ultimate extrapolation to classical states is not entirely evident, for it is precisely in the very small domains of space (structure of the nucleus, structure of the electron, and its trembling motion) where its present axioms break down. Quantum mechanics does not constitute an argument against causality.

However there comes news from other quarters which may upset the validity of the causal principle. If, as has been reported, the velocity of light, which is an essential parameter in the differential equations of physics, undergoes a slow variation in time, then a revision of the postulate may be in order.

Another significant objection has long been known although it is ordinarily overlooked. It has to do with spatial continuity of the universe. If the structure of nature's elements is continuous in space, and hence infinitely detailed, the equations representing the behavior of any of its parts will of necessity contain an infinite number of variables. Hence causal analysis, as it pro-

[2] H. Margenau, The Monist, Jan. 1931; ibid. April 1932.

ceeds into finer and finer details of structure, will meet the same obstacle which prevented it from exploring an inseparable universe as a whole. Two means are available for avoiding this difficulty; both have been employed. One is to adopt a field theory which fixes minutely the values of a physical quantity at every point of space, but fixes it by means of simple functions so that the scientist is enabled to dominate in one grand sweep all the intricacies of spatial structure. There are reasons, however, why this procedure is unsatisfactory. Unless the field functions used in physical theory are periodic they imply singularities which, while they are insignificant as far as many properties of nature are concerned, certainly do not exist.—The other and probably the better way to escape the difficulty is to eliminate its roots, that is to abandon the conception of a continuous universe. This is done in quantum theory by assuming the discrete existence of electrons, protons, neutrons, energy quanta and the like. The magic formula here was to reduce the number of variables appearing in causal description by endowing finite parts of space with homogeneity, so that this finite portion requires no more elements of description than does a point. Here again, quantum theory comes to the rescue of the causality principle.

Yale University.

Concerning the Validity of Aristotelian Logic

BY

L. KATTSOFF

THE controversy which has raged between those who accept Aristotelian Logic and those who reject it, seems to continue. It is, I believe, advisable to consider the arguments on both sides. Modern Logicians for the most part insist that Aristotelian Logic breaks down, i.e. it is not true for *all* meanings of the terms. On the other hand there are those who insist on the validity of Aristotelian Logic claiming that those values for which it fails are not permissible values. But there is one who stands almost entirely alone in his position—namely Professor H. B. Smith—who maintains that Aristotelian Logic breaks down because and only because its interpretation into Boolean terms has been accomplished by means of a definition that is not forced upon us and which, since it gives up desirable principles, is to be rejected.

The controversy begins with Leibniz who interprets the categorical forms in terms of existence. The battle rages through the years under the imposing title of "The Existential Import of Propositions."

Leibniz in many places explicitly mentions the fact that the assertion of the categorical forms implies existence, while in other places he assumes that it does tacitly. Thus for example, Leibniz says:

"Ut si dicam de re existente *A* est *B*, idem est ac si dicam *AB* est existens."[1]

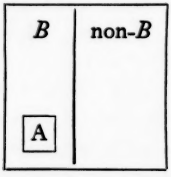
And then Leibniz interprets the four categorical forms as follows:
all *A* is *B*—the universal affirmative—as "omne *A* continens *B* est"
some *A* is *B*—the particular affirmative—as "*AB* est"
no *A* is *B*—the universal negative—as "*AB* non est"
some *A* is not *B*—the particular negative—as "*A* non-*B* est"
and in the following pages remarks that *A* and *B* exist.

But though Leibniz does recognize the null class yet he explicitly says after writing the universal affirmative as $C = BC$.

"Sunt et in omissione difficultate nam utique quia $C = BC$ non licit facere $O = B$" (p. 309).

Obviously enough if Leibniz's interpretation is accepted, then we are led to a breakdown by the following considerations.

If we were to assume that "all *A* is *B*" means *A* non-*B* non-Ens, then we can picture the fact as



which clearly shows that *A* and non-*B* have nothing in common. So that if *A* loses members until it becomes equal to zero, since at no step do *A* and non-*B* have anything in common, then at the limit *A* and non-*B* have nothing in common, and it is still true that "*A* non-*B* non-Ens." In other words, if all *A* is *B*, *A* non-*B* does not exist whether *A* exists or not. Yet when *A* vanishes it becomes false that "*AB* ens"—and we have subalternation invalid in spite of the validity of "all *A* is *B*."[2]

But this occurs on the *assumption that we do interpret the forms in existential terms.*

[1] "Opuscules et Fragments Inedits de Leibniz," par L. Couturat, Paris, 1903, p. 375.

[2] A similar interpretation is made by Whitehead and Russell in the forms $A(ab) = (x)\varphi(X) \supset \psi(y)$; $I(ab) = \exists x.\ \varphi(x) \supset \psi(y)$ this actually stating that "*there exists an X such that* (φX) is true." Cf. also Russell in Mind 14, 1905, p. 398, where Russell clearly states the existential import of particulars. $\exists$ means "there exists."

Although Leibniz's work was so important, it was relatively unknown until recently. The real impetus to Symbolic Logic was given by George Boole. But a significant factor in the work of Boole was that he realized the importance of Aristotelian Logic and attempted to define the forms so that substitution of 0 and 1 values would not destroy the desired moods. This he accomplishes by means of his symbol "v" which is introduced for a definite reason, even if it is vaguely defined. As Boole himself admits, he does not feel Aristotle's logic sufficient for all purposes, but there is no question in his mind of its validity.[3] Boole does not directly consider the problem of the existential import of propositions, but he does show how to deduce "some a is b" from "all a is b" by his method. Since he was well acquainted with 0 and 1 (which conceptions he got from DeMorgan), he certainly must have attempted to substitute these values in his formulas. In a previous chapter Boole devotes much space interpreting expressions involving the null-class which seems to imply that he clearly recognized its importance (p. 80ff). And certainly Boole was acquainted with DeMorgan's "Formal Logic."

DeMorgan writes "all X is Y" as $X)Y$ and "no X is Y" as $X.Y$ and the negative of any class X is written as x. DeMorgan argues as follows. Everything is either Y or y so that if we say that Y does not exist, then we must say everything is y. If then in $X)Y$, X does exist and Y does not, then we have $X)y$, or $X.Y$, i.e., $X)y$ is false. And on the same grounds it would be false that "some X is Y," and we get a false proposition implying a false proposition—which is permissible on modern grounds—and subalternation is valid.[4] Now, continues DeMorgan, suppose Y exists (and hence y by presumption; since Y cannot be everything else, what follows would not be valid) but not X. Then it becomes clear that "$y)x$ is certainly true but not $X)Y$, for when x is, as here, the whole universe, the proof of $y)x = X)Y$ fails to present intelligible ideas, that is fails to be a proof. But $Y)x$ or $Y.X$ is true."[5]

[3] Cf. Chapter XV on "Aristotelian Logic" on Boole's "Laws of Thought," London, 1854, p. 226.

[4] Formal Logic, A. DeMorgan, 1847, p. 111.

[5] Ibid., p. 111.

It is apparent that DeMorgan almost stumbles on a new categorical form—i.e., $x)Y$ (later[6] symbolized by U) but due to the fact that he has said X does not exist it follows that since $x)Y$ and everything is x, that $Y)x$ or $Y.X$ holds. But on this interpretation DeMorgan is himself led to a contradiction because from what is given, Y is equal to x or $y = 0$, since nothing is left for y to be, because $Y)x$ and $x)Y$. That, however, contradicts the hypothesis and DeMorgan's interpretation is invalid. We feel that this was apparent to DeMorgan since he passes over the matter in two lines. But of course he is trying to retain Aristotelian principles and hence his interpretation although tending to accept the existential import, is such that even where non-existence does enter there is no breakdown.

In 1884, Keynes[7] made a careful study of the question, indicating precisely what principles of Aristotelian Logic break down when existence or non-existence are assumed. We shall consider some of his results later—but the upshot of his investigation is that *universal propositions do not imply the existence of their elements while particulars do.*[8] However, Keynes, who agrees with Venn in this respect, recognizes that such a doctrine makes subalternation invalid, and he adds "this objection is far more than counterbalanced by the fact that A and O, E and I are now true contradictories" (p. 158). For Keynes, A implies I if and only if the subject of A exists. It is obvious that this question troubles Keynes as it did Venn because it implied the invalidity of certain principles that should be valid. Keynes adopts Venn's position because it seems to him that this retains most of the principles in question and where it is necessary to reject a principle there results a corresponding advantage elsewhere. But neither Venn nor Keynes made any attempt to extend the definitions of the forms so that all principles hold under all conditions. Both feel too that the generality of the terms should not be restricted. For all this it would have been extremely difficult to reach a satisfactory definition in purely verbal form—even symbolists seem

[6] Smith, *Primer of Logic*, (1917), Appendix II.
[7] Formal Logic, J. N. Keynes, 1887, p. 137 ff.
[8] Ibid., p. 155, and see below.

to have difficulty in extending their definitions (p. 138). Venn believes that if Symbolic Logic had been invented before Aristotle had taken such a hold upon us, A implies I would never have been admitted as a necessary implication.[9] This is like saying that if Non-Euclidean geometry had been invented before Euclid had taken possession of our thought, parallel lines would never have been defined or allowed in Euclid's sense.

Similar arguments might be recounted, but they all come to the same objection, which we may recapitulate for the last time.

It is assumed that the categorical forms are to be defined only in some fashion such as:[10]

$A(ab)$	as	$a \supset b$	(All A is b)
$E(ab)$		$a \supset -b$	(No A is b)
$I(ab)$		$a - \supset -b$	(Some A is b)
$O(ab)$		$a - \supset b$	(Some A is not b)

And it is said since a and b are variables the forms must hold true for all values of a and b even to the limiting values *the empty class and the universe.* To object that the null class is an artificial insertion or an impossible conception is to overlook the interest of generality. Since we wish to call the common part of two classes a class, we must find a way to interpret the common part of two classes when they have nothing in common. For this reason the concept is necessary and forced. If a becomes universe, how is $-a$ to be interpreted when it appears in our formulas?

[9] Venn attempted to symbolize I by $XY = 0$ which meant X and Y exists and refers to Leibniz *A.B.* ens. "Symbolic Logic," London, 1894, p. 184. Venn also recognizes the breakdown of subalternation, but says that I follows from A only on the hypothesis that the subject of A exists. It is interesting to note that in 1879 Venn maintained a different position "When ... we say that 'All A is B,' do we imply the existence of A and B? Certainly we do....

It is clear therefore that what we do is to take a license or make a convention for convenience sake....

In an affirmative proposition the subject and predicate distinctly imply the existence of these objects, but as we must appeal to experience to make sure of the existence of their contradictories, we have no right to contraposit such a proposition without due inquiry" (Mind, IV, 1879, p. 41).

[10] Cf. also the interpretation of A. Padoa in Rev. de Meta. et de Mor., V 20, 1912, p. 64 ff.

On this interpretation of the four forms we are led to infer that *the particular proposition implies the existence of membership in its classes.* And to substantiate this argument the following is reputed to show that without this limitation the valid mood Darapti becomes invalid:

$$A(ba)\ A(cb) < I(ca)$$

is to be expressed in this symbolism as

$$(b \supset a)\ (c \supset b) < (c - \supset -a)$$

Now suppose there are no members of the classes a, b, c i.e. they are all = o then we have

$$(o \supset o)\ (o \supset o) < (o - \supset 1)$$

since the negative of the null class is the universe.[11] And since anything is included in itself and the null class is included in every class we have the false proposition: a conjunction of two true propositions implies a false proposition.[12]

"This is one of the first and most remarkable results of the adoption of a logical ideography" said Padoa—except that the thing to be remarked, as it will turn out in the sequel is that *Aristotle was right* and that this interpretation need not be correct even if a very natural one.[13]

It becomes an interesting problem in personal Psychology to discover precisely what motives are behind this insistence on the invalidity of Aristotelian Logic when existence is not assumed. Perhaps it is the reaction that came as a result of too great dependence on the word of Aristotle. Certainly modern symbolic logic stands on its own results entirely apart from this supposed

[11] Despite McColl's insistence that the null-class is *excluded from every class*, and hence cannot be included in any class but itself. Cf. Symbolic Logic, London, 1906.

[12] To show that even today the same argument in almost the same words persists, reference can be made to Lewis and Langford, "Symbolic Logic," 1932, pp. 51–52 and 62, and in the new work of Korzybski, "Science and Sanity," Lancaster, Pa., 1932, pp. 141–42.

[13] How natural the existential interpretation is and how clearly it follows Leibniz can be seen by reference to Couturat "Des Propositions Particuleres" in Rev. de Met. et de Mor., 21, 1913, p. 256.

breakdown of Aristotle. What happens to modern logical theories if a new set of forms can be found by which Aristotle is vindicated? Such a set will of course be forced to retain modern views. And the forms which Prof. H. B. Smith offers do precisely all they are required to do. We shall repeat these later—and it will be seen that they really are an extension of the definitions given above.[14]

We are faced with two alternatives 1) either Aristotelian logic is invalid 2) or our interpretation of the categorical forms is incorrect. In the latter case it will become necessary either to redefine our interpretation or to extend our definitions. It has been usual to say that Aristotle's logic is invalid, but we believe Dr. Smith is incorrect is insisting that what is needed is an extension of definition. Only the authority of Leibniz seems to direct us to interpret Aristotle as has been done.

The extension of definitions to take care of what appear to be exceptional cases is a very common occurrence in mathematics. And this fact makes it more amazing that the second alternative should have been overlooked by many who were competent mathematicians. No attempt has been made to redefine the four categorical forms in such a way as to take care of Aristotle's logic, until the work of Dr. Smith.

In order then to uphold the breakdown of Aristotelian logic something like the following conditions (not necessarily independent) must be demonstrated:

(1) *No* definition is possible which satisfies all the conditions of Aristotle's logic and yet retains the validity of the disputed moods.

(2) Aristotelian inclusion is not translatable consistently into Boolean inclusion.

(3) Aristotelian Logic is inconsistent within itself.

(4) Aristotelian Logic and modern Logic are contradictory systems.

[14] Is it not possible that the whole argument began as a result of a misunderstanding of the mediaeval distinction between "rational" and "natural" existence? The scorn with which moderns treat the mediaevalists has perhaps obscured the real origin of the problem. Certainly if one wishes to consider the problem from the point of view of Ontology, such a distinction must be made (and is made even today).

(5) Aristotle's postulates cannot be deduced from the Boole-Schröder calculus.

Since conditions (1), (2), (4) and (5) are refuted by means of Dr. Smith's forms (which will be given shortly), we shall consider for a moment the question of the consistency of Aristotelian Logic.

So far as the author knows no one has troubled to exhibit the consistency of Aristotelian Logic—apparently because if it is invalid, it must be inconsistent somewhere—until the article of Professor Smith in the Monist of April 1932.[15]

Of course, what Professor Smith does is to *exhibit* the consistency of this logic—and if we accept the principle that an inconsistent system has *no* application, we must accept the inner consistency of Aristotelian logic. And this follows despite the article of Prof. F. C. S. Northrop "An Internal Inconsistency in Aristotelian Logic" in Monist for 1928. In this article Professor Northrop argues as follows:

Given the propositions

(1) All just acts are expedient acts
(2) All inexpedient acts are unjust acts
(3) Some unjust acts are inexpedient acts
(4) All unjust acts are expedient acts

Since the predicate of (1) is undistributed it may be possible that "all unjust acts" fall within that part of "expedient acts" and therefore (1) and (4) may be true together *at least once.* However (2) is obtained from (1) by contraposition, (3) from (2) by limitation—therefore (1) implies (3). But (3) is the contradictory of (4)—therefore (1) and (4) can *never* be true together. Therefore (1) and (4) are both consistent and inconsistent—and Aristotelian principles are invalid.

This is an example of how verbal forms can give rise to ambiguities and conceal fallacies. Professor Northrop's analysis of the apparent contradiction is beside the point and is based on the usual assumption that Aristotle's forms of necessity imply existence.

[15] Cf. also the appendix I to Dr. Smith's "First Book in Logic" 2nd edition (1933) with the new chapters on *Logic as the Art of Symbols.* We do not here reproduce the consistency proof because it can easily be consulted in the original text.

If (1) and (4) are true together by the analysis Prof. Northrop gives, then clearly the class of "inexpedient acts" is a null class and the whole universe is only "expedient acts." But if that is so and we consider "just" and "unjust acts," we do not have contradictories but merely contraries.[16] If "just acts" and "unjust acts" are not contradictories, contraposition cannot be performed. The analysis of Professor Northrop proceeds by taking "just acts" and "unjust acts" to be contradictories where as actually they are contraries. The analysis we have made was suggested by throwing the four given propositions into symbolic form. It then becomes evident that for (1) and (4) to be consistent the predicate must be the universe. From that it follows that "just" and "unjust acts" do not exhaust all the "expedient acts." Professor Northrop's analysis is hence invalid and must even be invalid in independence of *our* analysis, since the consistency of Aristotelian logic has been exhibited.

The forms of the categorical propositions as defined by Dr. Smith[17] are:

$$A(ab) = (a < b)\,[(b < a) + (a < b')'\,(b' < a)']$$

which may be read as follows:

a is in *b* and *b* is in *a: or else: a* is in *b* and it is false that *a* is in non-*b* and also false that non-*b* is in *a*.

$$E(ab) = (a < b')\,[(b' < a) + (a < b)'\,(b < a)']$$

i.e., *a* is in non-*b* and non-*b* is in *a: or else: a* is in non-*b* and it is false that *a* is in *b* and also false that *b* is in *a*

$$O(ab) = (a < b)' + (b < a)'\,[(a < b') + (b' < a)]$$

i.e., it is false that *a* is in *b: or else:* it is false that *b* is in *a* and true that *a* is in non-*b: or else:* it is false that *b* is in *a* and true that non-*b* is in *a*.

$$I(ab) = (a < b')' + (b' < a)'\,[(a < b) + (b < a)]$$

which can be easily interpreted.

[16] A concrete example will show that there are expedient acts which are neither "just" nor "unjust."

[17] Cf. appendix to 2nd edition "First Book in Logic" and "Symbolic Logic."

The meaning of the various symbols is apparent from the verbal readings.

We shall show below that these four forms do all they are supposed to do. With these *A* implies 1, and the definitions of contradictories, contraries, etc., are satisfied and the principles of obversion, contraposition, etc., are also valid. It becomes obvious that *Aristotelian Logic is still valid on this interpretation.* Whether or not there are other interpretations which make Aristotle valid, is irrelevant—here is *one* that does!

Of course we are not maintaining that Aristotle gives these forms but only that his instinct was correct and all the principles Aristotle insists upon are true. Aristotle nowhere commits himself on the existential import of propositions but from the tenor of his philosophy propositions about non-existent things would appear to him irrelevant. Still one cannot but feel, that Aristotle would have concurred that the validity of the syllogism is independent of the existence or non-existence of its subject or predicate. It is in this sense namely that all his principles are retained that we can call our logic Aristotelian.

Prof. E. J. Nelson has remarked quite correctly "All critically constructive discussions of this problem (i.e., of the square of opposition) start out from this last point (i.e., that there is an existential element in the traditional square)."[18] Recognizing that there are apparently some discrepancies involved in such a view, Dr. Nelson himself *sets out to find where the existential element is hidden.* In other words, Professor Nelson has seen the assumption and it has conquered him. His solution of the difficulties involved is to put the existential import outside the propositions, and to say that "the subject exists" implies "the square of opposition." Thus the difficulties outlined by J. N. Keynes *seem* to be avoided. That his analysis is also entirely unnecessary will become evident from the following considerations.

With the forms given above existence or non-existence is immaterial and the square of opposition still holds. It is obvious that for all ordinary meanings of *a* and *b*, *A*, *O*; *E*, *I*; are contradictories (i.e., they cannot be true together and cannot be false

[18] E. J. Nelson, "Square of Opposition," Monist, April 1932, p. 271.

together) since in each case we have contradictory factors. To demonstrate these assertions we shall make use of certain principles of the class calculus:

(1) o is included in any class (o is the null class, 1 is the universe).
(2) $o' = 1$; $1' = o$.
(3) If a is included in b, then b′ is included in a′; and the principles of the propositional calculus.
(4) If $y < z$, then y z′ implies an impossibility.
(5) The falsity of a true proposition is a false proposition and the falsity of a false proposition is a true proposition.
(6) A true proposition + a false proposition is a true proposition.
(7) A false proposition × a true proposition is a false proposition.

If $a = o$, $b \neq o$, $b \neq 1$

$A(ab)$ is $(o < b)\,[(b < a) + (o < b')'\,(b' < o)']$ is false
$O(ab)$ is $(o < b)' + (b < o)'\,[(o < b') + (b' < o)]$ is true
i.e., *$A(ab)$ is false and $O(ab)$ is true*

If $a = o$, $b = 1$

$A(ab)$ is false and $O(ab)$ is true

This and the following results are attained by actual substitution in the definitions. It was not thought necessary to show the actual work except in the first case as an illustration. Notice there is no opportunity for verbal ambiguities. If $a = o$, $b = o$

$A(ab)$ is true and $O(ab)$ is false

If $a = 1$, $b = c$

$A(ab)$ is false and $O(ab)$ is true

If $a = 1$, $b = 1$

$A(ab)$ is true and $O(ab)$ is false

If $a \neq o$, $a \neq 1$, $b = o$

$A(ab)$ is false and $O(ab)$ is true

If $a \neq o$, $a \neq 1$, $b = 1$

$A(ab)$ is false and $O(ab)$ is true

If $a = 1, b \neq 0, b \neq 1$

$A(ab)$ is false and $O(ab)$ is true

We tabulate the results obtained by actual substitution in the tables which follow and indicate under the name "vanishing factors" those factors which are impossible.

a	b	$A\ (ab)$	$O\ (ab)$	VANISHING FACTORS
0	$\neq 0, \neq 1$	false	true	$(b < 0)$ and $(0 < b')'$
1	$\neq 0, \neq 1$	false	true	$(1 < b)$
0	0	true	false	$(0 < 0)'$
0	1	false	true	$(1 < 0)$ and $(0 < 0)'$
1	0	false	true	$(1 < 0)$
1	1	true	false	$(1 < 1)'$
$\neq 0, \neq 1$	0	false	true	$(a < 0)$
$\neq 0, \neq 1$	1	false	true	$(1 < a)$ and $(0 < a)'$

Therefore A and O are contradictories even for the limiting cases. By a similar process we get the following table for E and I:

a	b	$E\ (ab)$	$I\ (ab)$	VANISHING FACTORS
0	$\neq 0, \neq 1$	false	true	$(b' < 0)$ and $(0 < b)'$
1	$\neq 0, \neq 1$	false	true	$(1 < b')$
0	0	false	true	$(1 < 0)$ and $(0 < 0)'$
0	1	true	false	$(0 < 0)'$
1	0	true	false	$(1 < 1)'$
1	1	false	true	$(1 < 0)$
$\neq 0, \neq 1$	0	false	true	$(1 < a)$ and $(0 < a)'$
$\neq 0, \neq 1$	1	false	true	$(a < 0)$

Therefore E and I are contradictories, because whenever one is true the other is always false.

From these tables we can read the following results:

1) *A and E are contraries:* because whenever A is true, E is false so that they are never true together, although they are false together.

2) *I and O are subcontraries:* because whenever A is false, I is true; so that they are never false together although they are true together.

3) *A and I, E and O* are *subalterns:* because they are values for which they are true together (e.g., for *A* and *I*, $a = 0, b = 0$) and values for which they are false together (e.g., for *E* and *O*, $a = 0$, $b = 0$).

4) There are no values which destroy the above definitions.

5) If the predicate is the universe, $A(ab)$ can be true if and only if the subject is also the universe.

6) If the subject is the null class, $A(ab)$ can be true if and only if the predicate is also null.

7) Under the conditions in 5) and 6) $I(ab)$ is true.

It will be of advantage to follow Keynes's development[19] to show that with our definitions everything holds under all conditions. We let s stand for the subject p for predicate, s' and p' for their contradictories

$$(1)\ s \ddagger 0, p \ddagger 0, s' \ddagger 0, p' \ddagger 0$$

then (*a*) *A* is not simply convertible follows from the following:—if *A* were convertible then

$$A(ab) < A(ba) \quad \text{or}$$
$$A(ab) < O(ba) < 0 \quad \text{since } A'(ba) = O(ba)$$

$A(ab)$ is $(a < b)\ [(b < a) + (a < b')'(b' < a)']$

$O(ba)$ is $(b < a)' + (a < b)'\ [(b < a') + (a' < b)]$. Multiplying and omitting the vanishing factors:

$$(a < b)\ (a < b')'\ (b' < a)'\ (b < a)'\text{: which does not vanish}$$

(b) that *I* is convertible follows from a similar consideration and the fact that $(b < a') = (a < b')$.

(c) *E* is obverse of *A*, since $A(ab') = E(ab)$ by definition; likewise $O(ab') = I(ab)$. Substitution shows they have the same truth value.

(2) $s \mp 0, p = 0$ (a) Under this condition Keynes says the converse of *E* is not valid. Referring back to our table under this condition $E(ab)$ is false and $E(ba)$ is also false. And since a false proposition implies any proposition $E(ab)\ E(ba)$ is valid and therefore the conversion of *E* is valid.

[19] Formal Logic, p. 143 ff.

(*b*) "The contraposition of $A(ab)$ is not valid for it involves the conversion of E which we have already seen to be not valid"[20] says Keynes—but under our definitions the conversion of E *is* valid.

It is unnecessary to proceed further. By actual substitution and by reference to our tables, we can see that all Aristotelian principles remain valid. It is unessential to consider the actual existence or non-existence of subject and predicate. With this we satisfy a fundamental demand of any system that purports to be general, namely: no restrictions are to be laid upon the generality of the variables. Any system which does so cannot be considered general and hence cannot be accepted as a final development.

As a conclusion of all the preceding, it becomes obvious that *the question of the existential import of propositions is solved negatively once and for all by a suitable definition of the four categorical relations.*

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[20] Ibid., p. 144.

On the Method of Theoretical Physics*

BY

ALBERT EINSTEIN

IF YOU wish to learn from the theoretical physicist anything about the methods which he uses, I would give you the following piece of advice: Don't listen to his words, examine his achievements. For to the discoverer in that field, the constructions of his imagination appear so necessary and so natural that he is apt to treat them not as the creations of his thoughts but as given realities.

This statement may seem to be designed to drive my audience away without more ado. For you will say to yourselves, 'The lecturer is himself a constructive physicist; on his own showing therefore he should leave the consideration of the structure of theoretical science to the epistemologist'.

So far as I personally am concerned, I can defend myself against an objection of this sort by assuring you that it was no suggestion of mine but the generous invitation of others which has placed me on this dais, which commemorates a man who spent his life in striving for the unification of knowledge.

But even apart from that, I have this justification for my pains, that it may possibly interest you to know how a man thinks about his science after having devoted so much time and energy to the clarification and reform of its principles.

*The Herbert Spencer Lecture, delivered at Oxford, June 10, 1933. Reprinted by permission of the Oxford University Press, New York, Inc.

Of course his view of the past and present history of his subject is likely to be unduly influenced by what he expects from the future and what he is trying to realize to-day. But this is the common fate of all who have adopted a world of ideas as their dwelling-place.

He is in just the same plight as the historian, who also, even though unconsciously, disposes events of the past around ideals that he has formed about human society.

I want now to glance for a moment at the development of the theoretical method, and while doing so especially to observe the relation of pure theory to the totality of the data of experience. Here is the eternal antithesis of the two inseparable constituents of human knowledge, Experience and Reason, within the sphere of physics. We honour ancient Greece as the cradle of western science. She for the first time created the intellectual miracle of a logical system, the assertions of which followed one from another with such rigor that not one of the demonstrated propositions admitted of the slightest doubt—Euclid's geometry. This marvellous accomplishment of reason gave to the human spirit the confidence it needed for its future achievements. The man who was not enthralled in youth by this work was not born to be a scientific theorist. But yet the time was not ripe for a science that could comprehend reality, was not ripe until a second elementary truth had been realized, which only became the common property of philosophers after Kepler and Galileo. Pure logical thinking can give us no knowledge whatsoever of the world of experience; all knowledge about reality begins with experience and terminates in it.

Conclusions obtained by purely rational processes are, so far as Reality is concerned, entirely empty. It was because he recognized this, and especially because he impressed it upon the scientific world that Galileo became the father of modern physics and in fact of the whole of modern natural science.

But if experience is the beginning and end of all our knowledge about reality, what role is there left for reason in science? A complete system of theoretical physics consists of concepts and basic laws to interrelate those concepts and of consequences to

be derived by logical deduction. It is these consequences to which our particular experiences are to correspond, and it is the logical derivation of them which in a purely theoretical work occupies by far the greater part of the book. This is really exactly analogous to Euclidean geometry, except that in the latter the basic laws are called 'axioms'; and, further, that in this field there is no question of the consequences having to correspond with any experiences. But if we conceive Euclidean geometry as the science of the possibilities of the relative placing of actual rigid bodies and accordingly interpret it as a physical science, and do not abstract from its original empirical content, the logical parallelism of geometry and theoretical physics is complete.

We have now assigned to reason and experience their place within the system of theoretical physics. Reason gives the structure to the system; the data of experience and their mutual relations are to correspond exactly to consequences in the theory. On the possibility alone of such a correspondence rests the value and the justification of the whole system, and especially of its fundamental concepts and basic laws. But for this, these latter would simply be free inventions of the human mind which admit of no *a priori* justification either through the nature of the human mind or in any other way at all.

The basic concepts and laws which are not logically further reducible constitute the indispensable and not rationally deducible part of the theory. It can scarcely be denied that the supreme goal of all theory is to make the irreducible basic elements as simple and as few as possible without having to surrender the adequate representation of a single datum of experience.

The conception here outlined of the purely fictitious character of the basic principles of theory was in the eighteenth and nineteenth centuries still far from being the prevailing one. But it continues to gain more and more ground because of the ever-widening logical gap between the basic concepts and laws on the one side and the consequences to be correlated with our experiences on the other—a gap which widens progressively with the developing unification of the logical structure, that is with the reduction in the number of the logically independent conceptual elements required for the basis of the whole system.

Newton, the first creator of a comprehensive and workable system of theoretical physics, still believed that the basic concepts and laws of his system could be derived from experience; his phrase 'hypotheses non fingo' can only be interpreted in this sense. In fact at that time it seemed that there was no problematical element in the concepts, Space and Time. The concepts of mass, acceleration, and force and the laws connecting them, appeared to be directly borrowed from experience. But if this basis is assumed, the expression for the force of gravity seems to be derivable from experience; and the same derivability was to be anticipated for the other forces.

One can see from the way he formulated his views that Newton felt by no means comfortable about the concept of absolute space, which embodied that of absolute rest; for he was alive to the fact that nothing in experience seemed to correspond to this latter concept. He also felt uneasy about the introduction of action at a distance. But the enormous practical success of his theory may well have prevented him and the physicists of the eighteenth and nineteenth centuries from recognizing the fictitious character of the principles of his system.

On the contrary the scientists of those times were for the most part convinced that the basic concepts and laws of physics were not in a logical sense free inventions of the human mind, but rather that they were derivable by abstraction, i.e. by a logical process, from experiments. It was the general Theory of Relativity which showed in a convincing manner the incorrectness of this view. For this theory revealed that it was possible for us, using basic principles very far removed from those of Newton, to do justice to the entire range of the data of experience in a manner even more complete and satisfactory than was possible with Newton's principles. But quite apart from the question of comparative merits, the fictitious character of the principles is made quite obvious by the fact that it is possible to exhibit two essentially different bases, each of which in its consequences leads to a large measure of agreement with experience. This indicates that any attempt logically to derive the basic concepts and laws of mechanics from the ultimate data of experience is doomed to failure.

If then it is the case that the axiomatic basis of theoretical physics cannot be an inference from experience, but must be free invention, have we any right to hope that we shall find the correct way? Still more—does this correct approach exist at all, save in our imagination? Have we any right to hope that experience will guide us aright, when there are theories (like classical mechanics) which agree with experience to a very great extent, even without comprehending the subject in its depths? To this I answer with complete assurance, that in my opinion there is *the* correct path and, moreover, that it is in our power to find it. Our experience up to date justifies us in feeling sure that in Nature is actualized the ideal of mathematical simplicity. It is my conviction that pure mathematical construction enables us to discover the concepts and the laws connecting them which give us the key to the understanding of the phenomena of Nature. Experience can of course guide us in our choice of serviceable mathematical concepts; it cannot possibly be the source from which they are derived; experience of course remains the sole criterion of the serviceability of a mathematical construction for physics, but the truly creative principle resides in mathematics. In a certain sense, therefore, I hold it to be true that pure thought is competent to comprehend the real, as the ancients dreamed.

To justify this confidence of mine, I must necessarily avail myself of mathematical concepts. The physical world is represented as a four-dimensional continuum. If in this I adopt a Riemannian metric, and look for the simplest laws which such a metric can satisfy, I arrive at the relativistic gravitation-theory of empty space. If I adopt in this space a vector-field, or in other words, the antisymmetrical tensor-field derived from it, and if I look for the simplest laws which such a field can satisfy, I arrive at the Maxwell equations for free space.

Having reached this point we have still to seek a theory for those parts of space in which the electrical density does not vanish. De Broglie surmised the existence of a wave-field, which could be used to explain certain quantum properties of matter. Dirac found in the 'spinor-field' quantities of a new kind, whose simplest equations make it possible to deduce a great many of the properties of the electron, including its quantum properties. I

and my colleague discovered that these 'spinors' constitute a special case of a field of a new sort which is mathematically connected with the metrical continuum of four dimensions, and it seems that they are naturally fitted to describe important properties of the electrical elementary particles.

It is essential for our point of view that we can arrive at these constructions and the laws relating them one with another by adhering to the principle of searching for the mathematically simplest concepts and their connections. In the paucity of the mathematically existent simple field-types and of the relations between them, lies the justification for the theorist's hope that he may comprehend reality in its depths.

The most difficult point for such a field-theory at present is how to include the atomic structure of matter and energy. For the theory in its basic principles is not an atomic one in so far as it operates exclusively with continuous functions of space, in contrast to classical mechanics whose most important feature, the material point, squares with the atomistic structure of matter.

The modern quantum theory, as associated with the names of de Broglie, Schrödinger, and Dirac, which of course operates with continuous functions, has overcome this difficulty by means of a daring interpretation, first given in a clear form by Max Born:—the space functions which appear in the equations make no claim to be a mathematical model of atomic objects. These functions are only supposed to determine in a mathematical way the probabilities of encountering those objects in a particular place or in a particular state of motion, if we make a measurement. This conception is logically unexceptionable, and has led to important successes. But unfortunately it forces us to employ a continuum of which the number of dimensions is not that of previous physics, namely 4, but which has dimensions increasing without limit as the number of the particles constituting the system under examination increases. I cannot help confessing that I myself accord to this interpretation no more than a transitory significance. I still believe in the possibility of giving a model of reality, a theory, that is to say, which shall represent events themselves and not merely the probability of

their occurrence. On the other hand, it seems to me certain that we have to give up the notion of an absolute localization of the particles in a theoretical model. This seems to me to be the correct theoretical interpretation of Heisenberg's indeterminacy relation. And yet a theory may perfectly well exist, which is in a genuine sense an atomistic one (and not merely on the basis of a particular interpretation), in which there is no localizing of the particles in a mathematical model. For example, in order to include the atomistic character of electricity, the field equations only need to involve that a three-dimensional volume of space on whose boundary the electrical density vanishes everywhere, contains a total electrical charge of an integral amount. Thus in a continuum theory, the atomistic character could be satisfactorily expressed by integral propositions without localizing the particles which constitute the atomistic system.

Only if this sort of representation of the atomistic structure be obtained could I regard the quantum problem within the framework of a continuum theory as solved.

An Examination of the Quantum Theories. II

BY

WILLIAM MARIAS MALISOFF

THE BOHR FORMULATION

THE striking synthetic effort made by Bohr to unite radiation facts with atomic structure facts led to the formulation of his "quantum theory of atomic structure," which with a large group of subsequent developments, is now usually referred to as the "classical quantum theory." As a conceptual aid it takes over the idea of an "astronomical atom" consisting of a positive nucleus around which electrons revolve in definite orbits, more or less in the fashion of the solar system. More or less. The similarities and differences as a bit of description are interesting and impressive, but when the theory is reduced to its postulates—and it is this which interests us here—it sounds very much like a re-statement (for a purpose) of the Planckian decisions. Bohr gives the following two as fundamental:

Postulate A. An atomic system can, and can only, exist permanently in a certain series of states corresponding to a discontinuous series of values for its energy, and consequently any change of the energy of the system, including emission and absorption of electromagnetic radiation, must take place by a complete transition between two such states. These states will be denoted as the 'stationary states' of the system.

Postulate B. The radiation absorbed or emitted during a

transition between two stationary states is monochromatic and has a frequency ν, given by the relation $h\nu = E_2 - E_1$ or $\nu = \Delta E/h$.

As they announce themselves these postulates characteristically emphasize an aspect of discontinuity, side-step any direct mention of a mechanism of transition from one stationary state to another, and contradict a theoretical requirement of the ordinary classical theory. In contrast to Postulate B ordinary classical theory requires that the frequency of radiation emitted or absorbed by an oscillator should be equal to its own frequency or a combination of it with simple harmonics. It should also be noted that, although there is substantial progress in saying about the *frequency* that $\nu = \Delta E/h$, since Bohr was so eminently successful in explaining line spectra thereby, it is, nevertheless, formally equivalent to Planck's seemingly converse statement about the *energy* that $\Delta E = h\nu$.

Of the nature of a third postulate there is also Bohr's Principle of Correspondence. It is a more or less flexible requirement that in the limit (that is, for large wave lengths or low frequencies) the results obtained by the quantum theory must be in agreement with the results obtained by the classical theory alone. That automatically narrows the forms into which solutions of given equations should be cast, should there be latitude for choice,—a wise precaution, but really of no higher degree of logical consistency than the original blending of classical and quantum considerations by Planck. One must not get the impression that thereby Bohr had made the classical theory a special case or an approximation of a more inclusive quantum theory or vice versa. It was rather an invitation to many efforts to give the expression "in the limit" a rigorous mathematical sense and thus really make the two theories approach one another. Several such demonstrations have been produced for the new forms of quantum theory (wave mechanics) and classical theory. Our point, however, is that the derivation of some results that are even precisely the same by two theories is no guarantee that contradictory elements in their foundations have been eliminated, since it is known that two sets of assumptions containing some

mutually incompatible elements can yield some identical propositions.

The Correspondence Principle has been extended to *amplitude* and *polarization* as well as to *frequency*. Of course, the logical inadequacy remains, although a useful purpose is served. The useful purpose eventuates in the so-called "selection rules" fixing the probability of certain energy level changes. (This appears in the more recent form of quantum theory as a *mathematical* condition for the solution of the wave equation.) One may go even further, since the technique of the Correspondence Principle is applicable to perturbed states (the Stark effect due to electrical fields and the Zeeman effect due to magnetic fields). But, again, the logical inadequacies remain.

DEVELOPMENTS

The reader probably knows that in 1925–6 a completely new type of attack was launched by Heisenberg, Born, Schroedinger and Jordan. We shall discuss that in our subsequent studies. What remains to be said here is that, previous to this, every effort was made to retain the Bohr postulates on account of their frankly unexpected success, and in spite of a natural reluctance to abandon the classical. Some contributions may be described as attempts to "demonstrate" the Correspondence Principle in some one of its forms; some are theoretical refinements which any theory may receive; some are ingenious applications; some apply the new results of relativity to orbital motion of the electrons; some suggest that the electrons have "spin"; all produce some very useful result, the most grandiose of all being the construction of the entire system of the elements. Every confirmation, however, seems to beget a new discrepancy or anomaly in experimental application, and we have the not unusual spectacle of collapse in the midst of victory.

Ten years of investigation of orbits ring the changes from simple circles, through ellipses, to rosettes showing the precession of elliptic orbits due to the relativity correction for the high speeds of the electrons. Bohr originally describes the hydrogen atom (a single electron rotating about a single positively charged

particle, the proton) in terms of a series of possible circular orbits (stationary states) for its electron. The orbits are distinguished by integers and the energies in the various orbits (or at the various "levels") are as the squares of these integers. The normal orbit (level, or stationary state) is designated by the integer 1. If the integer is greater than 1 we have an "excited" state. The designating integer, or "quantum number" n (also known as the "principal quantum number") can range from 1 to infinity, if the atom would hang together. The discontinuities of stepping from one integer to another are the discontinuities of energy absorption and emission. It is easy to see how the next natural step on the astronomical model is the consideration of elliptical motion. Its description involves the specification of a new kind of quantum number, k, the angular or azimuthal quantum number, which can take on various integral values subject to certain restrictions (a selection principle). Since spectral lines are due to an excited electron (the "Leuchtelektron") falling from orbit to orbit directly or indirectly toward normalcy, we always have a ready source of data via the spectroscope to check the theory. The introduction of a relativity correction (precession of the orbits) by Sommerfeld leads to the checking of many lines otherwise left unexplained. This, however, does not stop the flood of new data on a larger variety of elements, which do not fit. At least two other kinds of quantum numbers have to be introduced to account for the distortions of orbits (or shifts in spectral lines) which occur on account of electrical and magnetic fields, the electrical and the magnetic quantum numbers.

In due time something disquieting happens. Alternative explanations appear. For example, the assumption of spin on the part of the electron explains as much, if not more, than the application of the relativity correction. What has become of the *necessity* for the latter? Can we admit two "correct" explanations which so thoroughly ignore one another?

The uneasiness concerning the lack of correspondence between the classical theory and the Bohr theory must not be forgotten. Thus, we should note at least two efforts to produce a resolution of the difficulties, made during that period. Rubinowicz

publishes a derivation of the selection principle for azimuthal quantum numbers on the basis of classical theory. At least he renders clear the necessity or advisability of postulating the conservation both of energy and of electromagnetic momentum. Ehrenfest proposes the so-called "adiabatic hypothesis" which says in effect that a transition retains a classical character till it can do so no longer, that quantization is not "spontaneous." The restriction set on the suddenness of "quantum jumps" would be very satisfactory if it were a deduction from general principles, but it is really only a postulate itself—a more distinctly stated demand than the Correspondence Principle, yet not inherently any more necessary.

A final oddity of the period preceding 1925–6, which we wish to stress, is the existence of a very fruitful generalization, which can not be derived from the Bohr postulates, and yet with the aid of classical quantum concepts leads to the derivation of the periodic arrangement of the elements, the Pauli Principle. This makes its appearance as an *empirical* generalization to the effect that in any one atom there cannot be any two electrons with the same four quantum numbers. The Pauli Principle is not an explanation itself but demands explanation.

The "classical quantum theory" explains much almost quantitatively and is to that extent still useful as a matter of convenience. Struggling with an inner contradiction it has been only the more fertile in uncovering many new and important problems for investigation. It is safe to say that were it not for its crop of paradoxes concerning electron orbits and jumps, the great present-day quantum critique of measurement, a fitting parallel to the relativity theory, would have been much later in arriving.

SUMMARIZING STATEMENT

1. The Bohr quantum theory, like the original Planck formulation, rests on an inconsistent blend of continuity and discontinuity requirements.

2. The various principles of correspondence and the adiabatic hypothesis are only postulates.

3. Alternative, yet mutually exclusive, explanations appear in the framework of the old theory.

4. A theory may accomplish much without explaining its own empirical generalizations and without being quite accurate or even consistent.

Foundations of Mathematical Biophysics*

Dedicated to Mrs. E. N. Rashevsky

BY

N. RASHEVSKY

Μηδειζ αγεωμετρικοζ εισιτω.

—PLATO.

He who knows not mathematics cannot know any other sciences; what is more, he cannot discover his own ignorance or find its proper remedies.

—ROGER BACON.

Ich behaupte, dass in jeder besonderer Naturlehre nur so viel eigentliche Wissenschaft angetroffen werden könne, als darin Mathematik anzutreffen ist.

—I. KANT.

I

MATHEMATICAL methods in biology occupy a somewhat peculiar position, and the attitude of many biologists toward them is similar to that of many practical engineers toward what is called pure scientific research. The modern progressive engineer recognizes the value of pure science, which seeks for truth regardless of any possibility of practical applications; yet he still frequently shows a definite dislike towards such investigations. The whole history of civilization demonstrates that discoveries which, at the time they were made, did not show any signs of the possibility of being turned into practical use, now lie at the basis of many useful engineering devices. As Professor R. A. Millikan has said, we owe our present-day civilization to Galileo. Every educated person, be he pure scientist or engineer, recognizes this fact. And yet the frame of mind of the "practical" man is not too receptive to investigations which do not show at least some remote prospects of practical applica-

tions. Unless he can see at least hypothetically some practical results of such research, he will not give it his whole-hearted enthusiasm. The ultimate criterion in the evaluation of any research still remains its practical use. Those pure scientists who have had close contact with what is called "industrial research" know only too well the truth of the above statement.

Many men of science may feel tempted to revolt against such an attitude. And yet such a revolt would be unwise, because the above attitude is rather deeply rooted in human psychology and its parallel is found even within the domain of pure science itself. The attitude of the practical man towards pure science in general resembles that of the pure scientist who is an experimentalist towards the more mathematical branches of his own field. The experimental scientist recognizes the values of the mathematical sciences. He knows that mathematical investigations which at first glance looked like mental gymnastics without any connection whatever with "reality," have led subsequently to formulae that predicted new phenomena. Thus many experiments have been rendered unnecessary. When confronted with a mathematical investigation, the empirical scientist will give due credit to its interest and even to the possibility of some remote future connection with experiment, a connection which he may at present fail to see. But he will not appreciate the investigation wholeheartedly unless he sees some immediate connection between the mathematics and the experiment; unless he is given a formula which he can at once proceed to check by means of a set of thermometers, respirometers, galvanometers, etc. He will consider any mathematical theory as a mere intellectual curiosity. Biologists "approve of mathematics only when they lead to simple formulae which can be easily tested experimentally," says an eminent biologist. We see here essentially the same "utilitarian" attitude as the one found among engineers.

"It is utilitarianism of a higher grade" one may say. True, perhaps, but utilitarianism it nevertheless remains.

This utilitarian attitude is found in various degrees. In physics we see very little of it. Some experimentalists may still say that they do not care about theorizing but are hunting for

"facts." The great majority however realize that this hunting for "facts" would be simply impossible if theory did not indicate the direction in which to look. In the more empirical sciences, such as biology, this utilitarian attitude is still very pronounced, and that in spite of the realization that physics has much to tell us in biology. Biophysics becomes of greater and greater importance every day. But in this ultramodern union of the two sciences only the empirical part of physics has been joined to biology. And this at a time, when physics has long ago outgrown its empirical stage and become in its methods a firmly established mathematical science. Truly, this is putting new wine into old bottles. Here and there we see scattered attempts to apply the mathematical methods in biophysics. But nowhere do we see the attempt to build a complete and consistent system of mathematical biology.

The excuse most often advocated for this is the tremendous complexity of biological systems and biological phenomena. But, as we have mentioned in several of our writings, this is no excuse at all. True, biological phenomena are *perhaps* more complex than ordinary physical ones. But even the latter are on their face so complex, that their complete mathematical treatment may appear impossible. And yet it is just the mathematical method of approach that enables us to see through that complexity.

The important thing in the mathematical method is to abstract from a very complex group of phenomena its essential features and thereby to simplify the problem. The more complex features are then taken care of gradually, according to the degree of their importance and complexity, as second, third, and higher approximations. True, by abstracting, we lose, so to say, contact with reality; but no harm is done by this *as long as we keep it in mind.* We thus see that the complexity of biological phenomena is rather an argument *for* the use of mathematical methods than *against* it. In the case of a simple phenomenon we may hope to understand it without the use of mathematics, by simple inspection. But in a complex case we are left hopeless without mathematics. It does not take much of a mathematical knowl-

edge to determine the area of a rectangle two meters long and one meter wide; this is a problem for a child in the fifth grade. But it takes a knowledge of integral calculus to determine the area limited by a segment of a parabola and two straight lines.

The non-mathematically minded biologist will however hold to his objection, that in using mathematical abstractions, we lose contact with reality. "You may derive formulae," he will say "which approximate fairly well to some of our experimental results. But what is the use of all that if in deriving them you start by considering systems which are *never found in reality* and which are more simple than the real ones? Are your formulae not merely intellectual curiosities?" To this we may reply:

Any treatise on celestial mechanics begins by a study of the problem of two bodies. From the equations of the problem the three laws of Kepler are derived; namely, that a planet revolves around the sun in an elliptical orbit with the sun in one of the foci; that the radius-vector drawn from the sun to the planet describes equal areas in equal times; that the squares of the periods of revolution of any two planets are in the ratio of the cubes of the major semi-axes of their orbits. The derivation of these laws by Newton formed the first triumph of mathematical mechanics. And yet, to use the same argument as above, in deriving those three laws we start with an assumption which we never find satisfied in reality. For where is there a system which consists of only two bodies? In our solar system the real conditions are much more complicated. Can we lay any stress on formulae derived by neglecting the mutual disturbances of the planets? The answer is: Yes, we can as a first approximation. The higher approximations will take care of the complications. We can go further and recall that even in the complete theory of planetary motions, which has reached a remarkable degree of perfection and led to the prediction of new planets, we start with a fundamental assumption that is not found in reality. We assume for the law of gravitation Newton's law of inverse squares. We now know however that this law has to be replaced by one much more complex. The true law is given by Einstein's equations of gravitation, which, when written down explicitly in ordinary

algebraic notations, yield ten equations with several thousand terms each. Those who find biological phenomena too complex for a mathematical method of treatment would do well to ponder on that circumstance.

A characteristic of mathematical method is that it is applied to scientific problems for its own sake, regardless of immediate contact with reality. The contact may or may not come sooner or later; but the value of a mathematical investigation is not affected. An individual mathematical problem in a given field may never contribute to an experimental verification. But the whole system of mathematical studies of the field in question will undoubtedly do so. An "experimentally useless" mathematical treatment may be a prerequisite to another one, which has more contact with reality. Many of the latter would never come without the former. The experimentally confirmed formulae for the deflection of light in the gravitational field of the sun and for the "red shift," would never have been possible without purely abstract speculations on the geometry of our space, on how a clock appears to two different observers moving with respect to each other with stupendous velocities, and on other similar considerations, which do not have their counterpart in nature and seem to be only intellectual curiosities. And the choice of *one* definite geometry for our space-time would never have been possible without a preliminary study by mathematicians of all kinds of *possible* geometries, regardless of any chance of their realization.

If we attempt to build a system of mathematical biophysics, we must keep all the above firmly in mind. Euclid said, "There is no royal road in geometry," and this applies to any mathematical science. To ask of the new science of mathematical biophysics results that would lead immediately to tangible experimental verifications, would be to require it to take such an impossible royal road. Besides we should be guilty of the utilitarianism that characterizes the attitude of the practical man towards pure scientific research in general.

It is reported that Professor Max von Laue once remarked that if an experiment does not agree with theory, so much the worse

for the experiment I do not know how true the story is. Very likely it is an exaggeration. But to my knowledge, Professor von Laue has never performed any experiments. All his researches were done by the paper and pencil method. One of the outstanding results of his researches was the discovery on paper of the diffraction of X-rays by crystals. And not only did this mathematical study of his lead to innumerable experimental studies which revealed the fine structure of matter; but less than 15 years later all large industrial laboratories were studying a number of practical problems by methods based on von Laue's discovery.

Similar considerations hold for the relation between mathematical and experimental research in any science, and particularly in biology. Mathematical biophysics studies all physically conceivable possibilities of what may happen in a biological system. It studies these without regard to whether the possibility in question furnishes *the* explanation of a given biological phenomenon. It studies all possible explanations. And only after such a study has given us a clear insight into all possibilities, can experiment decide which of the possibilities are found in nature.

II

The fundamental living unit is the cell. And the processes of cellular growth and multiplication are the most primitive and fundamental processes in biology, found even in the lowest stages of life. Hence in beginning to build our system of mathematical biophysics, we shall start with a physico-mathematical theory of those general phenomena. But how shall we describe a cell mathematically?

Not only has any cell, even the simplest one, a very complex structure, but there is a tremendous variety of different kinds of cells. The simplest ones, some bacteria, are apparently minute droplets of a mixture of proteins and lipoids, possessing a comparatively simple structure. Others are highly complex. With the exception of some bacteria, they all possess at least two structural elements: nucleus and cytoplasm. But the latter itself is far from being structureless and uniform. It may contain

vacuoles, mitochondria, fat globules, pigments, chloroplasts, etc. In other words, it is a mixture of a great number of structural elements, some of which are present in some kinds of cells; some in others. The cell, as in many plants, may possess a rigid wall, or again it may not.

As regards the function of the cell, again we are in the presence of an apparently hopeless complexity. Some cells; consume one kind of substance; some another. As a rule each variety of cell has its own specific chemical composition. Various kinds of cells secrete various substances. A number of varieties of cells have one thing in common, namely that they respire by consuming oxygen and giving up carbon dioxide. But then a number of cells do not need any oxygen, and produce carbon dioxide from fermentation of sugar.

As regards the mode of multiplication of cells, we find simple fission in some bacteria, budding in yeasts, mitotic division in higher cells and possibly some rare cases of amitotic division.

How then shall we proceed to make a mathematical study of this complex group of phenomena? The answer is: by picking out the essential features of it, by abstracting. What, however, are these essential features on which we can build our first approximation?

Though most cells have a distinct nucleus, others do not seem to have it as a single separate structure. Hence the existence of a distinct nucleus is not one of the absolutely essential things. The same holds for such things as vacuoles, plastids, etc. Neither can we consider as essential any particular mode of cellular metabolism. The same holds with respect to various types of cell-division.

Retaining thus only those things which are common to all known cells, we conclude that a cell is essentially a small liquid system, a drop, in which occur some chemical reactions that result in growth. The necessary substances for these reactions diffuse into the cell from the outside, with some of the products of the reactions diffusing from the inside out. This growing drop, whenever it reaches a critical size, divides into two, each half growing again and so on. Moreover, division is the only method

by which new drops may be produced. No such drop is formed spontaneously, although all necessary substances may be present in the surrounding medium. Omne vivum e vivo; omnis cellula e cellula.

We are thus led to a physico-mathematical theory of such droplets as a first approximation to a theory of the cell. And this is no longer a hopeless task.

Here again we shall start with a simplest imaginable case—a droplet having a spherical shape and consisting only of one phase. The substance or substances of which such a droplet or cell is constituted must be insoluble in the surrounding liquid; or else the droplet must be surrounded by a membrane impermeable to these substances but permeable to the substances which are taken up by the drop from the outside and which participate in the reactions producing the substance of the drop. A systematical investigation of such conditions of existence of a drop of this kind gives us a number of further requirements as to the physico-chemical properties of the cell and of the food-substances.[1]

The next step is to investigate the distribution of the various substances taken up by the cell (food-substances), and of those given up by the cell (secretions). Regardless of any specific assumptions that we may make either about the chemical nature of these substances or about the constitution of the cell, we shall find that the concentration of all food-substances inside the cell will first of all be in general smaller than outside; and next, that this concentration will always be larger at the periphery of the cell than at the centre. For the secretions the reverse holds true. How large these gradients and differences of concentration will be, depends on the particular assumptions that we make in regard to the diffusion constants of the cell substance, the permeability of the cell surface, and the character of the chemical reactions involved.[2]

Thus if we assume that the coefficient of diffusion for a food- or secretion-substance is uniform throughout the volume of the cell, and that the reaction of "taking up" the food-substances or

[1] Zs. f. Phys., *53*, 107, 1929; Protoplasma, *14*, 99, 1931.

[2] Zs. f. Phys., *48*, 513, 1928; Protoplasma, *14*, 99, 1931; Physics, *1*, 143, 1931.

"production" of secretion-substances goes on at a constant rate independent of the concentration, then the law of variation of the concentration will be given by a parabola. If we assume a monomolecular reaction whose rate is proportional to the concentration of substances involved, then the variation of concentration of a food-substance will follow a law involving hyperbolic functions, and the variations of concentration of secretion-substances will follow a law involving trigonometric functions. All this holds if the cell grows sufficiently slowly; or, more strictly, if during the time that is required to establish a stationary diffusion state, the size of the cell does not change appreciably. A rough estimate shows that this condition is almost always satisfied in actual living cells.

In all cases the difference between the average concentration of a substance inside the cell and the outside concentration increases with increasing size of the cell.

The conditions of growth of a cell can be readily stated mathematically by requiring that the total amount of food-substances consumed per unit time should exceed that of secretion-substances produced.

We thus find that as the cell grows in size, the distributions of concentrations of various substances contained in it, necessarily change also. This change affects some of the physical properties of the cell. Our next problem is therefore to investigate what physical properties will thus be modified and in what way.

One important property of any dissolved substance is its influence on the surface-tension of the solution. This influence is sometimes very pronounced and is usually noticeable at very small concentrations. Let us then first consider the case in which all concentrations in question are very small, so small as not to affect any property of the cell except surface tension.

The latter will then vary with the size of the cell. The law of variation will depend on the laws governing the variation of concentrations of the dissolved substances, and on the laws governing the influence of these substances on the surface tension. Here a great number of cases are revealed for a mathematical investigation. The cases that present a particular interest are

those in which the surface tension of the cell, within a certain range of cell-sizes, increases very rapidly with the size. If a spherical cell divides into two halves, the total surface of the system will be multiplied by 1.26. Under ordinary circumstances, when the surface tension is constant, this means that the total surface energy increases to 1.26 times its former value. It takes energy to divide such a cell. In the above-mentioned case, however, when the surface tension increases very rapidly with the size of the cell, *it may happen* that when the cell divides into two, the surface tension decreases to less than $\frac{1}{1.26}$ of its former value. This would mean that in spite of the increased surface, the total surface energy, which is given by the product *surface* × *surface tension*, will have decreased. In other words in this case the division of the cell liberates energy and therefore, according to the laws of thermodynamics, it may happen spontaneously.

We now have a definite mathematical problem: to specify the conditions under which a cell will divide only above a critical size, and divide into two equal parts.

A survey of the possibilities in this case in connection with some known properties of catalytic substances, shows that this will happen when catalytic substances are produced in the cell. The necessary concentrations are very small, and the cell size is strongly affected in this case by minute quantities of such substances.[3] We have here one possible cause of cell division. We see however that though such a division is physically possible, it will only occur under rather special conditions. There is no reason why in some cases a cell should not grow indefinitely without dividing.

Let us now pass to our second approximation, and consider cases of sufficiently large concentrations of various substances in the cell. In this case the osmotic forces produced by the dissolved molecules play an important rôle. As we have seen, the difference in concentrations outside and inside the cell *always* increases with increasing cell size. This produces forces that

[3] Protoplasma, *15*, 427, 1932.

tend always to divide the cell so as to reduce this difference. Unless we have the rather special conditions mentioned above, the effect of surface tension will be to counteract this tendency. The first effect prevails for large sizes, being proportional to the volume of the cell. The second, being proportional to the surface, prevails for small sizes. Hence the cell will divide *above* a critical size. And this result holds quite generally regardless of any specific assumptions that may be made about the cell. All that is necessary is the difference of concentration outside and inside, which is caused by the processes of metabolism. Therefore, the fact alone that every cell metabolizes some substances must *necessarily* lead to its division, when it exceeds a certain size.[4]

This critical size can be calculated in such terms as the diffusion constants, permeability, temperature, surface tension and rate of metabolism. In order to apply the results of this calculation to any specific case, we must know all constants for all substances involved, which is impossible. This, however, is not necessary. An inspection of the formula for the critical size shows that only those substances have to be taken into account, whose rate of metabolism is high. A survey of actual conditions in living cells shows that the rate of respiratory metabolism by far exceeds all others. We may therefore neglect the others and consider only the respiratory metabolism. In this way we obtain for the radius of the cell the order of magnitude of 0.03 mm. That is the correct value. Moreover most of the constants in the formula enter in the $\frac{2}{3}$th power. A considerable variation of these constants will therefore produce comparatively small variations in cell-size. As a matter of fact, almost all cells vary only within the limits of 0.1 to 0.001 mm. in spite of the great differences in their physical constitution.

Another consequence of the theory is that cells with higher metabolism should be smaller, other conditions being equal. This checks in general with observational data. In the human body liver cells are the largest, and have the lowest oxygen con-

[4] Protoplasma, *16*, 387, 1932; Physics, *2*, 303, 1932.

sumption. There is evidence that the small cells of the upper cortical layers of the brain have a higher rate of metabolism than the larger cells of the lower layers.

A very small coefficient of diffusion for oxygen should produce very small cells. There are indications that for some bacteria this coefficient of diffusion is much smaller than usual.

It is also possible to calculate approximately the time a cell takes to divide. For small cells, especially for those without distinct nuclei (bacteria), it varies between 20 sec. and a few minutes, in agreement with observations. If in our calculations we take into account at least two phases of the cell (nucleus and cytoplasm), the division time is found to be several hours, also in agreement with observations on some eggs.

Is the cause of the cell division discussed above the only one? That we cannot say. But we can affirm that regardless of any possible influence, any cell, because of the very fact of its growth through intussusception, is bound to divide above a critical size. The theory as outlined, holds only for single phase cells, which are never found in nature. The consideration of more complex cases makes the calculations of the final results much more difficult, but does not affect the main argument.

As a next step in the mathematical study of cell-biophysics we consider non-spherical cells. At first glance such a thing as a free non-spherical liquid drop is impossible. A mathematical investigation reveals however that this is not so. It is possible, that in a liquid drop, which has a non-spherical shape, the distribution of concentration inside will be such, that although the surface tension will vary along the surface, from point to point, the product of the surface tension by the mean curvature at this point will be everywhere the same. In such a case the capillary pressure will be uniform, and a non-spherical shape that of equilibrium. This however will hold only as long as there are concentration gradients in the cell, as long as there is metabolism, as long as the cell is alive. A liquid cell which is not metabolising must of necessity be spherical. Note that free cells, which possess a non-spherical shape, actually round up as soon as they die (Euglena). It may also be mentioned, that this problem

leads to entirely new problems of a purely mathematical character, hitherto not considered by pure mathematicians.[5]

So far we have not touched the question why spontaneous generation of a cell should be impossible. Here also a mathematical study indicates some interesting possibilities. For instance, if a dissolution of the cell-substance occurs only at the surface in immediate contact with the surrounding medium, whereas the formation of the cell-substance takes place in the whole volume of the cell, then the first process prevails at small sizes, the second at large ones. Below a certain size a cell will not be able to exist, because its rate of growth is negative. It cannot therefore form spontaneously, because a very small drop would at once dissolve. Above the critical size the drop can exist and grow and produce new drops by division. Other possible causes have also been investigated.[6]

The next approximation in the mathematical theory of cell biophysics is the consideration of two-phase and in general polyphase cells. The difficulties here are much greater, and less definite results have been reached. However a mathematical theory of "indirect division" has been given. The inner phase (nucleus) divides spontaneously, in the above described manner. The energy liberated in this process may be used up to divide the outer phase (cytoplasm), whose division follows that of the inner one. Conditions under which this occurs can be formulated mathematically. As a possibility, an incomplete division may occur, leading to polynucleated cells.[7]

The gradients of concentration inside the cell produce forces on various parts of the cell. Of these forces we shall speak more in detail later on. Here we may mention, that these forces tend to arrange the two phases either eccentrically or concentrically. Under certain conditions, which also can be formulated mathematically, a complete division will result in an equal distribution of phases between the daughter-cells. And whenever such conditions are satisfied for a polyphase cell, the inner phases will

[5] Zs. f. Phys., *56*, 297, 1929. Physics, *1*, 143, 1931.
[6] Zs. f. Phys., *59*, 558, 1930; Protoplasma, *14*, 99, 1931; Physics, *1*, 143, 1931.
[7] Protoplasma, *15*, 427, 1932.

arrange themselves during the process of division in *the equatorial plane of the dividing cell* (cf. the arrangement of chromosomes during mitosis).

Some interesting relations are found in studying two-phase cells in which one phase is produced directly from the food-substances while the other is produced from the first. Under conditions similar to those studied in the question of spontaneous generation of drops, a two-phase cell of such a kind exists only if the "self-regenerating" phase of it is inside of the other. In other words the inner phase (nucleus) will regenerate the outer phase if the latter is removed, but not inversely.

As a next step in our system of mathematical bio-physics comes the theory of irritability, this being the next most general property of cells after growth and multiplication. A systematic deductive theory of this class of phenomena is still wanting. In this field attempts have been made to work out theories immediately applicable to definite empirical data, and perhaps this attitude is partly responsible for a lack of a very deep insight into the general nature of the phenomena of irritability. The future program should consist in an abstract mathematical study of various physically possible cases. Nevertheless certain features of irritability happen to find already an adequate mathematical representation. Since the classical studies of Nernst,[8] who first emphasized the rôle of the ions in the process of excitation by electric currents, a number of very important mathematical studies have been made by A. V. Hill,[9] K. Umrath,[10] and in a less detailed but more systematic form by P. Lazareff.[11] All these investigations confine themselves to cases of one kind of ions. Generalizing the mathematical treatment so as to include the cases of two different kinds of ions, one exciting and another inhibiting, we succeed in making a considerable step forward. The phenomena of excitation at the anode by opening the current, the non-excitability with slowly rising or falling currents,

[8] Pflüg. Arch., *122*, 275, 1903.
[9] Journ. of Physiol., *40*, 190, 1910.
[10] Biologia Generalis, *1*, 396, 1924.
[11] Theorie ionique de l'excitation des tissus vivants, Blanchard, Paris, 1928.

as well as several phases of electrotonic phenomena can be included in a natural way in the generalized theory.[12]

Another aspect of the mathematical theory of irritability is the theory of conduction and propagation of excitation. Although other processes of propagation known in physics are described by differential equations, the adequate mathematical representation of propagation of the nervous impulses is a simple type of integral equation. In those cases where the equations can be readily solved, it leads to an expression for the velocity of the nervous impulse in such terms as the size of the nerve fiber, the excitation threshold, etc., which is found to be in agreement with experimental data.[13]

Thus far we have considered individual cells. The next step is to consider the interaction between several cells. In what way is an interaction possible at all?

By absorbing food-substances from outside and by secreting other substances into the surrounding medium, every cell produces concentration gradients in its surroundings. For the food-substances the concentration in the neighborhood of the cell has a gradient *towards* the cell. The reverse holds true for secreted substances. Variations in concentrations produce variations in osmotic forces. If another cell is placed near the first one, the distribution of concentrations around it will not be symmetric, and as a result of it a force of *repulsion* will arise between the two cells. This holds quite generally, regardless of any special assumptions about the cell. The presence of metabolism is responsible for that repulsion.

A close inspection of some expressions derived previously in the theory of cell-division shows, that if a cell is placed in a medium, which contains a substance affecting the permeability of the cell, and if the concentration of this substance is not uniform, a force will act on the cell tending to move it in such a direction as to increase its permeability. More generally the cell will move towards regions in which its permeability increases, even if this increase is produced not by a dissolved substance, but by any

[12] Protoplasma, *20*, 42, 1933.
[13] Physics, *4*, 341, 1933.

other agent, such as a non-uniform electric current, etc. It is an empirical fact, that, no matter what cells we consider, excitation is always connected with increased permeability. Why this is so, we do not know, and for the present discussion, we do not need to know. It follows however from this fact and from the above mathematical conclusions that two irritable cells, which in some way or other influence each other so as to excite mutually, will attract each other.

Almost all cells are irritable, to a varying extent. All cells metabolize. Therefore the two forces, that of attraction and that of repulsion, are always present. Which one prevails depends on the circumstances. In highly irritable cells the attraction prevails. (Formation of ganglia in higher animals; diffuse nervous system of the lower.)

If we consider an aggregate of cells, which is derived by consecutive division from one cell, and in which the irritability is originally low but gradually increases, we find that in the early stages of development of such an aggregate, it will assume, under the influence of the above forces, shapes familiar to the embryologist, such as morula, blastula, gastrula, neurula. The further development of these forms will vary with varying amounts of general irritability. Arranging all possible forms in a series according to an increasing degree of irritability and increasing amount of irritable cells, we find a remarkable analogy with actual animal forms. The lowest, almost shapeless, correspond to mollusks, with a very low developed diffuse nervous system; others to arthropods, with several pairs of limbs and more concentrated ganglia; the highest to vertebrates with two pairs of limbs and one chief ganglion mass, the head. The least irritable cellular element, the red blood cells, lead a quasi-autonomous existence in the organism, being only loosely connected with it.[14]

It may be recalled, that the *general* shape of any adult organism is determined in early embryonic stages, when cells can still move with respect to each other under the influence of any force.

The above discussed forces between cells are not the only ones

[14] Protoplasma, *20*, 180, 1933.

possible, though they are those that *must* occur quite generally. Other possible forces and their influence on the organic forms have also been studied mathematically.[15]

The problem of simpler cellular aggregates may be also approached in a somewhat different way; namely, by considering a system with many phases and a very large specific surface, so that the surface energy is by no means negligible, as compared with the total energy. The usual phase rule, which is derived for negligible surface energy, has to be modified here. Such systems possess very interesting properties. Under some rather general conditions they exhibit phenomena of differentiation, increase in complexity, when increasing in mass. Parts of them, separated from the whole system, may regenerate into complete systems, etc.[16]

In the mathematical treatment of such systems, the thermodynamical method has been used. That is to say, such quantities as entropy or free energy are calculated in terms of the parameters, which determine the configuration of the system; and then such values of those parameters are determined as bring the corresponding quantities to an extremum; that is, such as give a maximum for the entropy or a minimum for the free energy. In this way the configuration of the system is determined.

A very usual case in such complicated systems is that the entropy or free energy possesses not one maximum or minimum, but several. Physically this means that the system has several configurations of equilibria, and that therefore the specification of the environmental conditions (such as temperature, pressure or anything else that affects the system) does not specify the configuration of the system. Such cases are not unknown to ordinary thermodynamics, but there they are considered exceptional and have not been thoroughly investigated.

A general mathematical study of such systems shows that they are all characterized by one important property; they all exhibit what is known as hysteresis. This means, that the configuration of such systems at a given moment is not determined by the

[15] Jour. Gen. Physiol., *15*, 289, 1932.
[16] Zs. f. Phys., 53, 107, 1929; Protoplasma, *14*, 99, 1931.

environmental conditions at that moment, but by the previous "history" of the system. Such phenomena as magnetic, colloidal and elastic hysteresis are only simple cases of more general hysteresis, and can be traced also to the existence of several equilibria in molecular and atomic configurations.[17]

The same change in environment will cause different changes in the system, depending on the configuration which this system possesses. Speaking in more physiological terms, the reactions of such a system to the same environmental change, will vary; they will depend on its "history" or, to be still more anthropomorphic, on its "previous experience." In a formal way this however is a characteristic of the behaviour of all organisms, particularly of the "higher" ones endowed with a highly developed brain. This dependence of reaction on previous experience we attribute to learning. And, from a purely formal point of view, learning is nothing more than a particular kind of hysteresis.[18] Thus our systematic mathematical study of bio-physical phenomena has led us in quite a natural, we may say almost synthetic, deductive way, from the elementary general properties of unicellular organisms to a mathematical study of behaviour of higher animals and man!

In accordance with our general principle, we should again refrain from attempting to give at once a theory of given psychological phenomena, but should rather first investigate the problems first *in abstracto*.

At the basis of learning lies Pavlov's conditioned reflex. Formally, the conditioned reflex may be described as a kind of hysteresis, possessing the following characteristics:

The variation of an "environmental parameter" α does not produce any change in the system. The variation of another "environmental parameter" β produces a definite change in the system. After α and β are changed simultaneously several times, the variation of α alone produces the same change as the variation of β.

The problem arises of investigating mathematically systems

[17] Zs. f. Phys., *53*, 102, 1929.
[18] Jl. Gen. Psychology, *5*, 207, 1931.

that exhibit this type of hysteresis. Such an investigation leads to a formal representation of a great number of characteristics of conditioned reflexes, such as experimental inhibition, extinction, trace reflexes, etc.

The next step is to study the simultaneous variations of several environmental parameters and their "conditioning." In this way we arrive at a study of systems which possess the property of discriminating response to spatial and temporal "gestalts".[19]

We have said, that every system possessing several equilibria exhibits hysteresis. But the reverse does not hold. Not all phenomena of hysteresis are due to the existence of several equilibria. These other types of hysteresis have also been mathematically investigated, and turned out to be of interest from the biological point of view. Various phenomena of fatigue have a close formal analogy with them.

Having established a general theory of conditioning, learning, and "gestalt" discrimination, we may try to connect the last with the previous studies of individual cells, so as to create a basis for a theoretical study of brain phenomena on a cytological foundation. Most interesting results are obtained in this way.

First of all, the forces of attraction between highly irritable cells lead upon closer investigation to phenomena that have already been described by various investigators under different names: Neurobiotaxis (Kappers), Neurotropism (Cajal), etc. The theory leads also to the prediction of a rather irregular, branched shape of the central nerve cells. If we consider this phenomenon as the basis of learning, and combine this with our previous results on the changes in concentration of the exciting and inhibiting substances in the nerves, we obtain a consistent mathematical theory of conditioning, inhibition, irradiation, extinction, fatigue, rebound phenomenon, etc. Learning curves of known type follow immediately. Also the general non-existence of "reversed reflexes."

The application of the general theory of conditioning to "gestalts" leads now not only to "gestalt discrimination" but to

[19] Jour. Gen. Psychology, *5*, 368, 1931.

"gestalt transposition" (a square no matter what its position on the retina, and whether black on white or white on black, or otherwise, is recognized as a square). Grouping of a number of elements into "gestalts" and the similarity between sensations of different quality, find a physico-mathematical representation.

Consideration of still more complicated cases leads to a mathematical investigation of the interaction between various nervous centers, and to a description in mathematical terms of phenomena of spontaneous activity and volition. It leads also in a further approximation to a physico-mathematical classification of different characters. And since at the base of all this lies the theoretical physics of the cell, which has also furnished us with a general theory of organic form, we obtain as the last synthesis, a mathematical connection between character and bodily constitution!

We have now arrived at a temporary halting place in our journey through the domain of mathematical biophysics. A great deal has to be done in the future to complete the edifice of this new science. A number of important problems have not yet been touched at all. In the mathematical theory of the cell we shall have to come down from very general considerations to more specific ones, to examine properties found in individual classes of cells. The theory of the nucleus and chromosomes with all their properties will have to be developed; and this will of necessity lead into the domain of atomic physics. "Atomic biophysics" may become a branch of general bio-physics. Problems of sex and reproduction of higher organisms will form a part of that program, though some hints have already been made in that direction. Mathematical bio-physics has just been born. And like a new-born babe it is as yet very undeveloped, but it contains in itself, in an embryonic stage, all its future qualities and characteristics.

The reader will have noticed, perhaps not without surprise, that throughout this paper we have not once used the words mechanism and vitalism. All the above results, as well as all those to come, are not in the least affected by the outcome of the mechanism versus vitalism controversy. So far we have not

found in biology any phenomenon that would prove entirely refractory to a physical interpretation. But some day we may perhaps find one. When this occurs, we shall see and discuss the situation. Any *a priori* discussions, based on anticipations, are waste of time. Inasmuch as *all* organisms are built of atoms, everything in them is ultimately reducible to description in terms of atoms, their aggregates and interaction. It may well happen that the laws of atomic interaction, given by our present atomic physics, may not be adequate to explain all biological phenomena. Then these laws will simply have to be revised. A mechanist will call that a generalization of physics, which enables the latter to include biology. A vitalist will call it a failure of physics and a triumph of vitalism. Whether that means much, we do not know.

The value and fate of mathematical bio-physics does not depend on such outcomes. It is an attempt to make biology an exact science. To many it will undoubtedly seem an intellectual curiosity, just as Maxwell's equations appeared to his contemporaries. The great electrical developments of our times are witnesses to what an intellectual curiosity may be worth.

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Time, Space and Gestalt

BY

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I. INTRODUCTION

TIME, space and matter are the most pervasive and inescapable aspects of the physical universe. And yet, notwithstanding the fact that they represent the most fundamental and ubiquitous characteristics of reality, they have always presented elements of mystery to the human mind. Thus on the level of common thought we ponder how the withering hand of time reaches from out the past into the future to bring decay and destruction to all things; and on the more sophisticated level, after the fashion of Immanuel Kant, we are puzzled by certain antinomies of thought concerning the necessity for an origin, and an end, of the universe in time and space, in the face of the equally necessary reasons for regarding the universe as being unlimited in its space and time dimensions. In either case, however, we are paying tribute to the tantalizing nature of these problems concerning the cosmic framework of space-time-matter in which we live, and move, and have our being. Indeed, it is not too much to say that genuine philosophy took its origin in Zeno's paradoxes of motion, and will have its final consummation when it solves Kant's antinomies.

It is well known that in trying to solve what we may term the time-space-matter problem there has been a persistent tendency in philosophy and in physical science to regard change as irrational and unreal. The ideal pattern for science, as Emile Meyerson has shown, may be provided by the Parmenidean conception of a timeless and changeless universe. And yet, whether

from the side of mere common sense, which ridicules Zeno's proofs of the impossibility of motion, or from the side of biological evolution, or from the irreversible changes in physics due to the second law of thermodynamics, change *will* force itself into the static picture and turn it at least into a movie film. How to bring together the conceptual world of thought and the perceptual world of growth and motion still remains one of the main problems of the philosophy of science.

But while these problems are still with us, it cannot be said that we have not made progress in our treatment of them. In earlier philosophy the question of the eternity of matter was not supposed to be connected in any way with the problem of the finite or infinite nature of space, or the limited or unlimited nature of time. Moreover, space and time were commonly supposed to be independent of the "matter" which "filled" them; space and time were supposed to be the container within which matter was put, without disturbing any of them. In classical physics this view appears in Newton's theory when he regards time and space as absolute, with each preserving its virginal nature throughout all cosmic vicissitudes.

Now all this is changed. We have learned that the old assumption—that these three aspects of the cosmic trinity, time-space-matter, are independent of each other—is false, and that if we are to speak in terms of a trinity we must introduce the notion of intercourse and fertility to understand the relations of the members of this union. For physics now presents the picture of a universe in which all three are intimately related. Since the work of Einstein, Newtonian physics has been radically modified; physical science has adjusted itself to the revised notions, and is rapidly forging ahead formulating new insights. But in philosophy the implications of the new physics have not been fully developed. It is well known that Kant's view, that time and space are *a priori* forms which the ego superimposes upon the external world, was based largely upon the universal acceptance of the Euclidian-Newtonian world picture. While physicists have largely emancipated themselves from this view, philosophers are still influenced by the set of philosophical habits

which Kant stabilized. But even in philosophy a reconstruction is in progress, the characteristics of which are especially manifested in such a thinker as A. N. Whitehead. In general there is a common trend towards an *organic* view of nature; an analogy is suggested, for example, in astronomy in the notion of an expanding (or expanding-contracting) universe, *the radius of which varies with time*. Does this not imply some organic connection between the members of the trinity, matter-space-time?

In the present essay an attempt is made to deal with the time-space-matter problem in terms of this organic view of nature. Since the present period of intellectual reconstruction provides little justification for finality, the view here presented must be taken as tentative and speculative. Before presenting this view it is desirable to define certain of the fundamental concepts which it will be necessary to employ.

II. DEFINITIONS

Since it is now recognized that space, time and matter are abstracted aspects of a universe that is spatio-temporal-material, it is clear that it is impossible to define one member of the trinity without implicitly introducing the other two. This inevitably leads to circular definitions,—unless we assume an intuitive understanding of the meaning of each term and leave them undefined. An *operational theory of concepts* does not escape this problem, but merely limits the range of application of the terms defined.

Space. Leibniz's definition of space as the order of co-existence of things may be restated as follows: space is the order of relatedness of positions of objects taken at any given instant from some point of view. Thus space is not a "receptacle" or "thing-in-itself." Space might be defined as the possibility of motion of matter, provided it is recognized that since motion is relative, space also is relative. Even the shape (spatial contour) of an object is relative: thus a spherical body appears to be flattened when viewed from a system moving at right angles to it.

Time. Physical time is the order of succession of external phenomena. It is the one-dimensional direction in which all

events are aligned. *Velocity* is the measure of the time-rate of change of motion. Velocity, like *acceleration* and *force*, is relative. Therefore, except for bodies in absolute contact, *now* ("simultaneity") is ambiguous. That is to say, *physical time* is a measure of the change of a system as observed from an external point of view, and varies from one "frame of reference" to another. But it is also assumed that in certain systems (discussed later) which undergo change, *this change is experienced subjectively as temporal passage.* This experience constitutes *psychical time*, and under these conditions "time is the mind of space," as Alexander says.

Matter. Matter is any pattern of electrical density which maintains itself in the same region of space in time. Some authorities have tried to make *energy* a more fundamental reality than matter; but if *matter* is a *substance* concept, and *energy* a *function* concept, then they (being "polar" concepts, as Morris R. Cohen would say) are correlative and inseparable.

Gestalt. A gestalt is a spatio-temporal organization, or pattern, of matter in which the relations are internal to each other, i.e., there is an interaction between the parts and the whole.

Organism. An organism is a whole composed of interacting gestalten. In this sense atoms and the cosmos itself may be organisms, of which space, time, and matter are abstracted aspects. J. H. Woodger[1] tells us that in order to have the wholeness characteristic of an organism there must be an internal relatedness (an inner necessity more or less autonomous, according to gestalt theory) between the parts and the whole. This is summarized by Professor Whitehead[2] in the following lines: "The concrete enduring entities are organisms, so that the plan of the *whole* influences the very characters of the various subordinate organisms which enter into it. In the case of an animal, the mental states enter into the plan of the total organism and thus modify the plans of the successive subordinate organisms until the ultimate smallest organisms, such as electrons, are

[1] "The 'Concept of Organism' and the Relation Between Embryology and Genetics," II, *Quart. Rev. Biol.*, 1930, Vol. V, p. 449.

[2] *Science and the Modern World*, 1926, p. 115.

reached. Thus an electron within the living body is different from an electron outside it, by reason of the plan of the body." Whitehead constantly stresses the fact that the notion of "nature at an instant" is an abstraction; that it requires time for a thing to exhibit its properties; and that a thing is "social" in nature, because it requires an environment to "be" itself. Thus the organic view avoids the fallacies of "simple location" and "misplaced concreteness."

III. PHYSICAL ASSUMPTIONS

The following theses summarize the naturalistic conditions which are regarded as necessary in order that there can be a *time sense:*

1. *There must be an organism in an environment.*

2. *There must be a relative motion of the environment* (or some part of it) *with respect to the organism.* This is illustrated by the Doppler effect, either for sound or for light, where the quality of the resulting sensation depends upon this relative movement. In the case of audition *a variation in motion of the observer with respect to the source of the sound, considered as stationary, produces the same effect* (raising or lowering the pitch, or frequency, of the sound, depending on the direction of the motion) *as an objective variation of the stimulus frequency when the observer is at rest with respect to the source of the stimulus.* Once we are *adapted* to a stimulus it usually requires another *relative* change of stimulus attributes to produce another sensation (pain being the only sensation to which we cannot become *accommodated*). Even the sense of motion in an elevator (due to the statoliths) depends upon a relative *change* of velocity.

3. *But not all motion yields* (causes) *a sense of temporal passage.* Those objects (e.g., a pendulum) which cannot discriminate change, because there is no *integration* of subjective rhythms, have no time sense.

4. *There must be some complexity of structure to produce a time sense.* By definition every organism has a time sense, and everything that has a time sense is an organism. The need for complexity of structure arises from the fact that experienced time is a

gestalt, like a melody. In man this complexity of structure involves both the *interoceptors* and the *exteroceptors*.

5. *There must be an interaction between subjective and objective rhythms.* While the complex organism has rhythms of functioning of its constituent organisms, the time sense emerges as a result of the interaction of internal and external rhythms. What is an internal rhythm from one point of view (e.g., the heart beat) may be an external rhythm from another point of view.

6. *In every organism that which is externally observed as a movement* can be said to be *internally perceived* (*experienced*) *as a passage of time.* Here we agree with Bergson that time as experienced and time as measured may not coincide quantitatively, but we deny Bergson's thesis that experienced time (duration) does not inhere in physical reality. As Josiah Royce suggested, we may not be able to communicate with lower organisms (organic and inorganic, for us) because our time sense, or rhythm of attention, may be different. We accept Kant's idea that the source of all synthesis (or "apprehension into unity," as Whitehead puts it) is subjective.

7. *The spatio-temporal unity of an organism* in a sense *is its mind.*

8. *The wholeness of every organic system has associated with it a subjective phase which is experienced as the passage of time.* This is due, (*a*) to the intrinsic nature of the component rhythms of the system itself, and (*b*) to the relation of this subjective unity to the external rhythms and events of the cosmic environment. Thus the time span of events, of *subjective duration* and *objective physical motions*, is relative. In general, as the writer has pointed out,[3] *there are three types of relativity:* (1) *physical relativity*, (2) *biological relativity*, and (3) *psychological relativity. Each of these represents a kind of "hysteresis" of the next lower level, which provides the structural basis of that relativity.* Thus psychological relativity is conditioned by biological constitution (a kind of phylogenetic hysteresis), while biological relativity is a special case of history as recorded in physical structures that have preserved some residuum of past environmental influences.

[3] "Relativity and Reality," *Monist*, Vol. 41, 1931, 512–534.

This results in a continuity which is a source of strength to a hypothesis.

IV. ORGANIC TIME

If the present view is correct, anyone who propounds a theory of biological and psychic time must assume some theory of the nature of time as it functions in physical science. The only other recourse would be to throw overboard the doctrine of evolution and hold that experienced time is of no kin to time as it enters in physical ("inorganic") phenomena. This doctrine that both orders, the objective (physical) and subjective (psychical) time orders, are ultimate and irreducible has of course been put forth. But this is to create a rift between the physical and the psychical, leaving us with a philosophy of nature containing a dualism similar to Bergsonian vitalism. In the faith that it is possible to develop a tenable theory of time in physical science, which will bridge the supposed gap separating "mechanical" behavior from "purposive" behavior, while at the same time providing a conception of the origin of psychic time, we enter upon this next section by considering first of all the various differentiating features which have been suggested as uniquely characterizing organic or vital systems.

Some biologists, attempting to find a criterion for distinguishing between animate and inanimate behavior, have found in the *historical* character of organic phenomena a differentiating property. Borrowing a term from Alfred Korzybski, the advocates of this view might say that organisms are *time binders*. Thus L. V. Bertalanffy[4] has argued that this historical character is quite foreign to physics, since physical equations are of such a kind that the state of the system for which they hold is determined solely by the present conditions of the environment, regardless of the previous history of the system.

By way of replying to this view, we may point out that this idea is flatly contested by N. Rashevsky,[5] who has shown that "learning by experience" may be a property of physical systems,

[4] "Philosophie des Organischen," *Lit. Ber. a. d. Geb. d. Phil.*, 1928, no. 17–18.

[5] "Learning as a Property of Physical Systems," *J. Gen. Psychol.*, 1931, Vol. 5, 207–229.

and that organic memory has its prototype in *hysteresis* phenomena in physics. A somewhat similar view has been presented by Maximillien Winter,[6] who has shown that there are cases in which heredity (i.e., past history) plays a rôle in mechanics.

A second criterion of organic processes is said to consist in the fact that the notions in biology of *metabolism*, *function*, *protoplasmic gradients*, etc., imply an asymmetrical temporal relation which has no duplicate in physical processes. Such statements as this, that "life is the sum total of forces making for irreversibility," have been taken to mean that physical transactions are two-way processes (to use G. N. Lewis's term), while organic processes imply a "one-way" time system. In disposing of this view we need only emphasize that time in physics has no single and unambiguous meaning, and that *some* physical processes, especially those which proceed in accordance with the second law of thermodynamics, may be unidirectional or irreversible. Moreover, such notions as "stellar evolution" and "atomic evolution" (inevitably suggested by the arrangement of chemical elements in the periodic table) certainly introduce the notion of the asymmetry of time, and this is not nullified by the recognition of the inverse phases of these processes.

The view here set forth inevitably poses the interesting question of whether time as it enters into biological processes is to be considered as *experienced* time, or the "*t*" of physico-chemical reactions. Here is an illustration of the problem: we know that an organism such as a paramœcium or frog will exhibit antagonistic ("avoiding") reactions if it is suddenly placed in a fluid medium of higher temperature than normal, but if the temperature of the water in which the organism is already situated is elevated slowly, even though the temperature reaches the point where damage to tissues is done, no "unpleasantness" apparently is felt. Here, as in the case of the human being, a *sudden* change causes a reaction, but the same change, *slowly* initiated, produces no, or a different, reaction. Is this temporal influence to be considered as analogous to something in the inorganic world, such

[6] "Time and Hereditary Mechanics," *Monist*, Vol. 35, 70–80.

as the bending of a bar of iron which breaks *if bent too rapidly*, or, in the case of the frog, is it part of the phenomenon of psychic processes, involving the experience of pain as a causal influence? Our own view is that these are two different ways of regarding the same facts: the objective aspect is the *t* of physical chemistry, and the subjective aspect is the experienced time of all organisms. It is undoubtedly true that mental phenomena are conditioned by the chemical nature of protoplasm, and if, in the above case, one is to attribute consciousness to a unicellular organism one must then eventually consider whether conscious stresses do not have their origin in the laws of energetics which govern all reactions (physicochemical and biochemical), and whether, indeed, when the physicist speaks of the "overstressed" and "understressed" molecules (or groups of molecules) of the iron bar which reaches its "yield" point at different times, depending upon the *rate* at which it is subject to stress, we may not even here have a physical basis for the stresses of psychic life.

We have stressed the fact that all continuants of nature are space-time-matter entities. These complexes of behavior-stuff, by a process of abstraction, are susceptible of analysis into spatial and temporal coördinates. In some cases it is convenient to concentrate on the spatial aspect of the polyphasic colloidal systems known as the protoplasms. Thus, as J. H. Woodger points out, anatomy studies the organism in timeless space. But this must not lead us to neglect the fact that the organism is an historical as well as a spatially extended thing. As a durational creature an organism is enregistered; in its engrams history is recorded. In a living organism more of the past is preserved than in a non-living organism. In man the phyletic effects of previous biological evolution manifest their influences through what von Hartmann and the psychoanalysts term the unconscious. The writer has tried to show how such psychic reactions as are present in religion[7] may be explained in these terms.

The most pervasive phase of the temporal coördinate of protoplasmic systems is necessarily associated with colloidal chemistry.

[7] Cf. "The Biological Origins of Religion," *Psychoanalytic Review*, 1932, Vol. 19, 1–22.

The facts in this connection have been well stated by Jerome Alexander[8] in the following passage:

> Figuratively speaking, if all chemical substances comprising our organism were in a true or crystalloid dispersion, reactions would proceed so rapidly that we would, so to say, live ten years in ten minutes. On the other hand, if coarse dispersion prevailed, it would take ten years to live ten minutes. Every organism is dependent on the coordination of its reactions *in point of time*, and this leisurely procedure depends largely on *degree of dispersion*, which keeps chemical reaction velocities within certain speed limits through its regulation of free surface and kinetic activity. Life lies between lysis and coagulation. The colloidal zone is, as it were, a vital metronome tolling off the tempo of life.

As biological evolution progresses time stamps its imprint deeper and deeper into protoplasm. Thus it comes about that as more intelligent organisms appear the ability to learn by experience supplements fixed hereditary modes of response. Viewed externally as a palæontological record, evolutionary time is the tempo of emergent evolution; viewed subjectively by human beings as the latest, if not the "highest" products of the process, experienced duration condenses the achievements of that phylogenetic advance. Every organ of a complex living thing functions rhythmically, and all the rhythms enter as constituent phrases into that larger melody which on the structural side is the complex human body. Normally each organ beats its time in harmony with the other players of the symphony. As Lancelot Hogben[9] says:

> It now appears that the organism is a self-recording clock. It registers intermittently the cyclical changes which occur in the external world. It registers without ceasing the manifold and ubiquitous cyclical phenomena which arise within its own proprioceptive field. In virtue of its visual and tactile receptivity to agencies in the external world, and of the proprioceptive influence of its eye muscles when exercising its organ of vision, an animal can orient itself in space. It is also a self-

[8] *Colloid Chemistry*, 1929, p. 25.

[9] "A Biological Analysis of Sensation," *Psyche*, 1931, Vol. 11, 26–47.

adjusting instrument internally recording its relation to the earth's gravitational field by means of the labyrinthine organ. To Kant space and time were essentially 'mental' constructions. This was inevitable because his view of the field of bodily sensation was purely exteroceptive. Hence he believed that space and time are absolute in the sense that duration and orientation can be separated from the physiology of sensation. From the standpoint of contemporary biology, the concepts of space and time owe their peculiar permanence to the circumstance that we can never get away from our own labyrinthine organs or the rhythms of our own bodies. The proprioceptive field is the most enduring part of the field of sensation. It is also the most inaccessible, and therefore the source of more erroneous philosophical speculations.

The subject of time in its relation to organic functioning has also been studied by Dr. Alexis Carrel,[10] and the following is quoted at some length to show what the results indicate:

The body really consists of a flux of structural and functional processes, that is, of uninterrupted modification of tissues, humors and consciousness. Such is physiological duration. The process of aging starts simultaneously with embryonic life. It is expressed by irreversible changes progressing during the entire span of our existence. The decrease in the rate of growth during infancy and youth, the occurrence of puberty and menopause, the lowering of basal metabolism and the modification of the skin and hair, etc., appear as the stamp of time on the organism. Most of these phenomena either occupy a relatively short period of our duration or are not susceptible of sufficiently precise measurement. Fortunately, other physiological and chemical processes taking place in tissues or in blood plasma have been found to be measurable during a considerable part of the life span. When small fragments of tissue are removed from an animal and placed in a medium practically deprived of nutrient substances, they manifest some activity and for a few days increase in size. The length of the period of growth and the velocity of the process can easily be ascertained. They express the residual growth energy of the tissues. In an embryo, this residual energy is greater than in a new-born animal. It continues to decrease during youth. But the aging of the organism can not be traced during the whole life by this method because the differences in the growth energy of the tissues of adult and old animals are too small

[10] "Physiological Time," *Science*, 1931, Vol. 74, 618–621.

to be accurately detected. Moreover, each type of tissue appears to record time in this own way. A more definite effect of time on living structures is obtained when one studies the variations of the rate of healing of a wound in function of the age of a patient [calculated by du Noüy's equation].

It is interesting to note that while Dr. Carrel believes that the process of aging remains irreversible (so that the rejuvenating experiments of Steinach and Voronoff must remain as barren as the older quest for the *elixir of life*), he nevertheless points out that physiological time has been found to be not quite inexorable, for by drying a rotifer one can temporarily stop the stream of its duration and by increasing the temperature of the environment of the fruit fly it can be made to age more rapidly. This suggests the possibility of control within certain limits, and this, coupled with proper mental hygiene, might enable a greater proportion of human beings in their old age to exclaim with Victor Hugo: winter is on my head, but summer is in my heart!

V. PSYCHOLOGICAL TIME

Not only is the temporal sequence of nature one of the most puzzling phases of the objective world—the sense of temporal passage is also one of the most baffling aspects of the subjective world of human experience. Like the problem of space perception, the problem of the perception of time remains one of the old, and ever-new, riddles of psychology. Professor W. B. Pillsbury[11] tells us that "much less is known definitely of the perception of time than the perception of space." This, we may remark, is all the more mysterious if it is true, as Pillsbury states, that "time is much simpler than space and has fewer components."

The sense of time is bound up with the idea of sequence, the irreversible series of *instants* which are so ordered as to enable us to recognize and place events as "before" and "after." The unidirectional character of experienced time is illustrated in several ways. For example, one is free to journey back and forth, e.g., from America to Europe, in space; but one cannot

[11] *The Essential of Psychology*, 1922, p. 190.

journey backward into last week, except in memory, and one cannot journey forward into next week, except in imagination,—though this latter statement might turn out to be false if such experiences as are recounted by J. W. Dunne in his book, *An Experiment With Time*, should be shown to be valid. A more technical illustration of the one-way character of psychical duration is found in music in the process of *chord resolution* in melodic and harmonic progressions. It is for this reason that playing a phonograph record backward takes the "meaning" out of it.

And now we ask, what is the origin of this remarkable time-sense?

Psychologists have debated this matter for some time. Various points of view are held, views which overlap on various issues and are not necessarily exclusive of each other. One phase of the complex problem of the origin, nature, and locus of the time-sense is given in the question, is this sense innate or acquired? In answer to this question we find here, as in the case of the parallel problem of the origin of space perception, the *nativistic* and the *empiristic* views. Kant, who struggled with the problem of passing from a *succession of perceptions* to a *perception of succession*, invoked the "transcendental ego" to explain the "synthetic unity of apperception." Kant held that the source of synthesis was subjective, and that an innate sense of time is the presupposition of all phenomena and experience. Since the days of Kant much progress has been made in biology and sensory physiology, and, under the influence of the empirical and behavioristic spirit which has grown so rapidly in recent years, Kant's views have fallen somewhat into disrepute. And yet I think it can be shown that in one respect Kant's view is inescapable. If we consider *sensationalistic* psychology, we find an attempt to build up the psychic continuum, the meaningful world of experience, by compounding psychic "atoms" (sensations, images, etc.) into more complex "molecular" mental states. But, be it noted, in this doctrine of "mental chemistry" we must begin with simple elements which already possess such attributes as "clearness," "quality," "intensity," and "*duration*."

This temporal voluminousness is accepted as an innate attribute of the elements of conscious experience. *Structural* psychology, which was the natural descendant of sensationalism, never tried to analyze further this temporal dimension of conscious states.

In recent years the empiristic movement has gained impetus as a result of the study of *conditioned reflexes*, a work largely initiated by Pavlov and further generalized by the Behaviorists. Influenced by this work, empiricists condemn Kant's views and assert that the sense of time develops as a conditioned reflex. In reply to this it need only be pointed out that to tie up the sense of time, or experience of duration, with some physiological rhythm does not avoid the nativistic view, for the original discrimination of that rhythm is still unaccounted for. In a similar way Herbert Spencer's notion of subjective time as an "echo" of objective time, developed through a process of evolutionary adaptation, is open to the same objection, aside from the difficulty confronting any theory of the "inheritance of acquired characteristics." In the last analysis, sensationalism, associationistic psychology, structuralism, and reflexology must agree with gestalt psychology and regard "temporal patterns" as wholes not compounded of parts. It needs to be pointed out that this view that a temporal whole is non-summative is not the same as that of W. Wundt, who regarded the perception of space as a result of a *creative synthesis*. At least for some gestalters, the whole exists independently of the parts into which it can be broken up.

Time perception, like the perception of *movement*, is unanalyzable. Hence Madison Bentley's[12] attempt to solve the mystery of the perception of movement by bringing it under his general principle of *coalescent integration*, according to which successive trains of stimuli undergo temporal ligation by being incorporated into a "movement complex," merely restates the difficulty. There appears to be no difference in principle between assuming a nativistic theory of time perception or a nativistic theory of the perception of movement. In either case we recognize that the experience of time is not of empirical (external) origin. This

[12] Cf. *The Field of Psychology*, pages 137 and 233.

view in no way implies that the empiricists have not made out a good case for the view that in space perception the response to a third dimension, or of *depth*, is a conditioned reaction involving a synthesis of visual, motor, and tactual space. On this point Berkeley's theory of vision is now commonly regarded as correct.

Perhaps the problem of the origin of the experience of time has seemed more difficult than the parallel problem of the origin of space perception for the reason that there is no known sense organ for time perception, whereas space perception is supposed to be more definitely localizable, e.g., in the thalamus, occipital lobe, motor cortex, etc. To be sure, each sense organ may be considered as a kind of clock. But in addition to this, it is believed by some that there must be a general sense for time perception independent of the special senses. Thus Julius Pikler[13] believes that just as there is a common organ for consciousness, so there must be a sense organ (*Zeitempfindungsorgan*) for time perception. Similarly the Russian physiologist, E. Cyon, who finds in the three semicircular canals the organs of the sense of space and the physiological foundations of Euclidian geometry, believes that Corti's fibers are the special organ of time perception. This view may be satisfactory for audition—though this is debatable—but certainly it cannot suffice for other types of temporal perception. For example, the fact that "filled" time (i.e., time during which we are interested in some series of objective events) should be short in passing but long in memory, whereas "empty" time is long in passing but short in memory, suggests that time here is discriminated largely in terms of bodily tensions and relaxations, and that consciousness of muscular sensations (*kinæsthesis*) plays the predominant rôle.

The relative importance of the sensory, cerebro-spinal, and motor aspects of the organism has been much discussed. In recent years some investigators have depreciated the importance of the central nervous system. But in connection with this problem of the perception of time it seems that William James was correct in seeking for the explanation of the locus of the time

[13] *Sinnesphysiologische Untersuchungen*, Leipzig, 1917, 485–495.

sense in some central process. James's view is interesting and bears repetition. The general explanation which James offers[14] takes its point of departure from the fact of the "specious present" of the organism. As James says, the practically cognized present is no knife edge, but a saddle-back, on which we are perched, and from which we look into the two directions of time. The number of units which can be brought under one act of consciousness ("span of attention") depends on the grouping of the units. According to James, twelve seconds constitutes the maximum filled duration (e.g., 40 strokes in rhythmical grouping of sounds composed of 5 sub-groups of 8 units). This grouping is possible because a knowledge of the adjacent parts of the "stream of consciousness," past and future, is always mixed up with a knowledge of the present. The intuition of duration, which James pictures as fairly constant for each passing instant, must be correlated with some fairly constant feature of the brain processes to which consciousness is tied. This brain process James finds analogous to the phenomenon of "summation of stimuli" and after-images. In neural terms, he states, there is at every moment a cumulation of brain processes overlapping each other, of which the fainter ones are the dying phases which but shortly previous were active to a maximal degree. The amount of overlapping determines the feeling of duration occupied. What events shall appear to occupy the duration depends upon just what the overlapping processes are.

Idealists have not been inclined to accept James's version of the unity of the self as the unity of the passing thought. Mental life, they seem to hold, exhibits the sort of thing which Bergson calls "interpenetration," which may not be capable of explanation in terms of such metaphors as "overlapping." James admits the limitations of his explanation when he confesses, "Why such an intuition results from which a combination of brain-processes I do not pretend to say."

While at the present time we know little more about the brain than James did, our knowledge in the field of physics has made

[14] *The Principles of Psychology*, Vol. I, Ch. XV.

tremendous strides, and it would truly be significant if the solution to James's problem should be contained in this new physical knowledge. To indicate what the possible liason between the physical and the psychical may be it is necessary to turn aside for a moment to consider the phenomena of vision.

It is well known that visual perception operates within definite temporal limits. The eye cannot see the details of objects, or the objects themselves, if they are in too rapid motion. Nor is it possible to see the movement of something, the rate of change of which is too slow. As an illustration of the first we have the revolving spokes of an automobile, and as an illustration of the second the growth of flowers. These can be made visible by the use, respectively, of the stroboscope and slow motion cinemetography. A similar situation arises if the objects are stationary and the eyes in motion. To overcome these difficulties the eyes, where possible, make leaps along the line of sight, pausing long enough at certain "rest" points to recognize the details of the objects seen. The duration of the pause varies with various factors, such as the size of the object seen, the "threshold-time" being shorter for large objects than for smaller ones. It also varies with the brightness difference between the object and its background, the minimum pause decreasing as the difference in intensity of illumination increases.[15] In most cases the minimal threshold time lies between a tenth and two tenths of a second. There is of course an absolute minimum for the amount of *space* (area), *time* and *brightness* below which vision is impossible.

It is interesting to inquire whether there is anything in common underlying the temporal factor in the various types of sense perception. It is known that in audition and vision parallels exist with respect to the amount of time and intensity necessary in the stimulus to cause a conscious response. Furthermore, in both cases there is a critical point in the rapidity with which sensations succeed each other, beyond which fusion (or the illusion of continuity) occurs. It is important to note that in the case of tactual discrimination an "illusion" parallel to the illusion of *seen* move-

[15] On this matter consult *Seeing*, by M. Luckiesh and F. A. Moss, 1931, Ch. VII, and *Principles of Experimental Psychology*, by Henri Piéron, 1929, pp. 78 ff.

ment (Wertheimer's *phi-phenomenon*) is found. One outstanding difference between vision and audition is presented by the fact that in vision the spatial coördinates predominate. It is for this reason that the "arts of space" are associated with vision and the "arts of time" (e.g., music) with hearing. However, these two should not be too sharply separated, for there is a psychological correlativity of the two in some cases.[16] Indeed, recent ideas growing out of the study of quantum phenomena indicate that it may be possible to interpret psychological phenomena in terms of a mental space-time manifold which is the subjective aspect of the four-fold continuum of physical relativity. To show this it is necessary to turn to physical considerations.

It is generally known that the fundamental unit of the four-dimensional world of relativity is termed *action*. Action is energy integrated through time. In the quantum theory of energy this dimension is known as the *erg-second*—a unit of energy multiplied by the unit of time. Quantum laws tells us that while the radiation (light) from different atoms (sodium, calcium, etc.) is of a different number of ergs and the periods of a different number of seconds, the product of the two contains the same number of erg-seconds.

Light of shorter periods of vibration (gamma rays, X-rays, ultra-violet radiation) contains the more intense energy. For this reason a small amount of high-frequency light (e.g., ultra-violet) can produce results on a photographic plate which light of a lower frequency (red) cannot produce. The quantum of energy for red light is 25×10^{-13} ergs; for violet light it is 50×10^{-13} ergs. A study of these facts in relation to the time it takes to "see" the various kinds of light brings out the interesting fact that *the minimum amount of energy necessary to cause a visual response not only depends on the wave-length but also on the time of exposure*. That is to say, the response to any color requires time as well as frequency, and quantum theory shows that this unit of "action" to which the retinal elements respond is approximately

[16] See Harry Helson's paper, "The Tau Effect—An Example of Psychological Relativity," *Science*, 1930, Vol. 71, 536–537. The quantitative study appears in *J. Exp. Psychol.*, 1931, Vol. 4, 202–217.

the same for all colors. This fact has been emphasized by E. E. Fournier D'Albe[17] in the following lines: ". . . it is a remarkable fact, recently established by very painstaking research, that the smallest amount of energy perceptible as sound by the ear is just about the same in amount as the smallest amount of energy perceptible by the eye. If the new 'quantum' theory applies to both forms of energy, it means that our senses have accomodated themselves to the minimum quantity of energy in existence *provided it comes to us in a constant succession of pulses frequent enough to produce an impression of continuity*" (italics mine). This is significant, for it indicates that just as the retina integrates the intensities of the monochromatic constituents of the light it sees into the total brightness of the complex light, so the brain (presumably) integrates energy through time. Perhaps we have here a clue to the solution of Kant's problem of how to pass from a succession of perceptions to a perception of succession, and to James's problem of how an intuition of duration can arise out of an "overlapping" of brain processes.

What we are here intimating is in harmony with the suggestion concerning the significance of the new physics for psychology. Quantum mechanics and relativity considerations *must* apply to organic phenomena if we are to consider the latter as falling within the domain of physical reality.[18] The only reason for ruling them out would arise in connection with the question as to whether psychophysiological processes are of the right order of magnitude with respect to the space, time and mass dimensions concerned. It is interesting to note that one *can* carry over some notions from physics to biology. Aside from the much discussed question of the bearing of physical "indeterminacy" (Heisenberg) on the psychological problem of the "freedom of the will," there is the fact that when you observe a phenomenon in physics you change it (due to the Compton effect) has its parallel in biology, where the attempt to study protoplasm in an intimate way results

[17] *The Moon-Element*, p. 90.

[18] On this subject see the author's papers on "Physical Relativity and Psychical Relativity," *Psychological Review*, 1930, Vol. 37, 257–263; and "Contributions of the New Physics to Philosophy and Psychology," *Psyche*, 1930, Vol. 11, 65–87.

in killing it. Moreover, the fact that the field of an electron extends to infinity, and that two electrons may therefore occupy the same space, suggests a remote physical analogy to Bergson's observation concerning the "interpenetration" of conscious states. Again, if one is to speculate more boldly, as Dr. Rashevsky has suggested to the writer, it is not impossible to interpret the entire organism in terms of wave mechanics, the fundamental rhythm of the organism being considered as the curve from birth to death, with the activities of the individual cells within that rhythm being treated as analogous to the "eigenfunctions" of wave-mechanics. Finally, the analogy justifies the use of such phrases as the "music of consciousness," the "melody" of an instinct, etc., since "particles" on the undulatory theory of matter appear as interference effects, like "beats" in music, due to the superposition of *group waves*.

It is true that the view which the writer has proposed,[19] that the unidirectionality of experienced time may be due to the irreversible character of the processes which take place in accordance with the second law of thermodynamics, which probably applies to biochemical reactions no less than to physicochemical, is not directly related to the doctrine of wave mechanics. And yet one may well anticipate that some day wave mechanical principles may be applied to thermodynamics. At least there can be no conflict, since the second law is purely phenomenological, in the sense that it tells us nothing about the microscopic or kinetic mechanisms concerned in chemical reactions. To work out the details of such a view one would have to work up from wave mechanics into the biological periodicities which coöperate in producing the conscious experience of duration. This would involve working towards a physico-chemical theory of nerve conduction, the "refractory" period of nerve response, facilitation and inhibition, and central integration.[20]

[19] In the following papers: "Probability, Natural Law, and Emergence," *J. of Philos.*, 1926, Vol. 23, 421–434; "A Phenomenological Interpretation of Physico-Chemical Configurations and Conscious Structures," *Ibid.*, 1927, Vol. 24, 373–385; "Light, Wave-Mechanics, and Consciousness," *Ibid.*, 1928, Vol. 25, 309–317.

[20] A beginning in this direction has been made in the following papers: "Biological Organization," by D. L. Watson, *Quart. Rev. Biol.*, 1931, Vol. 6, 143–166; "Quantitative

In such a theory the phenomena which L. Lapicque has described under the term "Chronaxy" would bulk large. This term refers to the synchronism which must exist between neuro-muscular cell groups if nerve transmission is to be accomplished. If each individual nerve cell has a characteristic frequency, nerve conduction would be a process whereby an excitation is passed from one cell to another with which it is in "tune" (or *isochronous*). Perhaps we may even apply the idea of *resonance* to explain the discrimination of spatial and temporal patterns. Here again the roots of this conception lie in the rhythmical character of the afferent and efferent nervous impulses. The writer[21] has proposed that the brain may be looked upon as a harmonic analyzer and synthesizer, and has suggested that the melody and harmony of conscious life is interpretable in terms of the consonance of the "fundamentals" and "overtones" of neurone periodicities. It must not be ignored that such theorists as L. T. Troland[22] see difficulties in this view of undulation-patterns. Nevertheless it must also be kept in mind that chemical periodicities are well known, that colloidal systems exhibit rhythmic phenomena, and that, as Dr. Rashevsky[23] has shown, a resonance between two periodical chemical reactions is possible, in the sense that their mutual influences increase with the decreasing difference in their proper frequencies. The writer has suggested that among the various types of biochemical reactions which may be responsible for these periodicities is the common oxidation-reduction rhythm.

It is now agreed that oxidation-reduction reactions are electrochemical in nature. Biologically this may mean that the differences in electrical potentials which undoubtedly exist are due to the bioelectric currents associated with cell oxidation. It is also inferred by some, P. Lazareff for example, that since electromotive forces are present in the brain, electromagnetic fields must

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Relations in Biological Processes and the Radiation Hypothesis of Chemical Activation," by C. D. Snyder, *Ibid.*, 1931, 281–305.

[21] In his booklet on *The Alchemy of Light and Color.*

[22] See Troland's treatise, *Psychophysiology*, 1932, Vol. iii, p. 121.

[23] "Sind resonanzerscheinungen bei physikalisch-chemischen Periodizitäten möglich?," *Zsch. f. Phys.*, 1930, Vol. 65, 270–272.

also accompany cortical reactions. Some would be reluctant to assign much importance to such fields of force, since they cannot be very strong, but we must not forget that such fields might be strong enough to initiate processes (by a "trigger" effect) which then proceeded on their own free (potential) energy. This conception may throw light on the problem of relating the unidirectionality of experienced time to the periodical-reversible reactions of bodily processes. If consciousness is related to field-forces, we may interpret the matter aspect of the brain as expressing the character of the future so far as it is determined by the happenings of the present, to use Whitehead's[24] words, in which case the one-way streaming of consciousness may be the streaming of the Faraday tubes of force through a field which extends into space and streams through time into the future, as Whitehead[25] says. The practical consequence of this idea is that such fields should be capable of detection by suitable means of amplification (thermionic tubes). The further theoretical significance of the notion can be brought out by reference to the views of the gestalt psychologists and the theories of L. T. Troland.

The gestalt psychologists were among the first of the theorizers to make use of the results of physical chemistry in interpreting psychophysiological phenomena. In considering the intimate nature of brain chemistry gestalt theory supposes that biochemical reactions resemble the reactions of dilute solutions, involving the migration of ions in accordance with electrolytic laws. We do not have space to go into this matter[26] here; suffice it to say that if the parallel between phenomenal (conscious) patterns and physicochemical patterns is to be made complete, the gestalters must discover some relation between psychophysics and electrochemistry. This task has been undertaken. By utilizing Nernst's theory of galvanic chains, Professor Köhler[27] is able to

[24] *The Principle of Relativity*, 1922, p. 9.

[25] *Op. cit.*, p. 76; see also the paper "Uniformity and Contingency," in the *Proceedings of the Aristotelian Society*, N. S., Vol. 23, 1923, pp. 1–19.

[26] The writer has tried to summarize the argument in a paper on "Gestalt Psychology and the Philosophy of Nature," *Phil. Rev.*, 1930, Vol. 34, 556–572.

[27] *Die physische Gestalten in Ruhe und im stationären Zustand*, 1920.

state the relation between the potential difference, Φ_1-Φ_2, and the concentrations, C_1 and C_2, as follows:

$$\phi_1 - \phi_2 = \text{const} \cdot \log \frac{C_2}{C_1}$$

This, Köhler points out, is exactly Fechner's formula for Weber's law, which states that the difference between visual sensations follows the law:

$$S_1 - S_2 = K \log \cdot \frac{R_2}{R_1}$$

Professor Köhler recognizes that the logarithmic law no longer refers to single sensations, but to the whole structure.

The writer has defended the view that the chemical basis for the production of potential differences may lie in the oxidative-reductive rhythms previously mentioned. Bioelectric potentials are in many cases redox potentials; minute actions currents are developed as the electrical charges are passed from the oxidizing to the oxidized body. Since the number of ions produced is determined by the *ratio* of oxidation to reduction, Köhler's derivation of the psychophysical law can come from an adaptation of Nernst's formula for the oxidation-reduction potential as given the familiar equation:

$$E = E_0 - \frac{R\mathcal{T}}{MF} \log \frac{[ox]}{[red]}$$

The difficulty with Köhler's conception is that the Weber-Fechner law is concerned with the perception of differences in *intensities* of sensation. This is only one variable in the sensory complex, and quality (color) still remains to be explained. In his recent book on *Gestalt Psychology* Köhler points out (p. 218) that in dealing with solutions with different ionic concentrations the answer to the question as to whether a given solution is "electronegative" or "electropositive" depends on the relation of this given solution to the other solutions with which it may be compared. Köhler does not identify the relativity of color qualities (e.g., as in simultaneous color-contrast) with the rela-

tivity of the ionic concentrations of solutions. He does, however, point out that differences in chroma are usually associated with differences in brightness, which act as effective "forces of separation." So far as I am aware, this is as close as Köhler has come to stating the physiological condition of color perception. Unfortunately E. D. Adrian's work on *The Basis of Sensation* tells us only that *intensity* is correlated with the *frequency* of nerve impulse, and leaves us with the problem of finding the quality in some central process. The investigator who has shown most audacity in speculating about this central process is the late L. T. Troland, who points out[28] that *there are only two possible means of dynamical unification in space, viz., electrical and magnetic fields (or lines of force); that* (p. 119) *physical structures which are minute below a certain point yield qualitative, rather than structural differences, on the psychical side; and that* (p. 120) *if a configuration of electromagnetic forces is sufficiently intense, or well integrated, the psychical parallel is not a pattern but a quality.* In commenting on the view of K. S. Lashley, Troland argues (p. 65) that a floating or wandering record within the cortex is practically inconceivable. But the present writer has pointed out that any "element" of a phenomenal pattern gets its character in part from its *relation* to other elements of the gestalt, and that since the gestalt, like a melody, can be *transposed*, conscious patterns with the same *logical structure* may well arise out of the behavior of different cortical units. From this point of view we differ with Troland because he seems to be defending the discredited doctrine of stabilized localization of cerebral function, and we agree with Lashley when he refers to patterns (dynamic) shifting about over nervous structure much as an electric sign over a bank of lights. The conscious side of the shifting patterns, as they progress through time, is associated with the field, while the biochemical basis resides in the ratios of the differences of potentials, which are relative and change with time.

The value of this view is that it frees the theorizer from the

[28] *Psychophysiology*, Vol. iii, p. 46.

poverty-stricken physiology of the past and thus makes it possible to find a functional complexity of brain configurations paralleling all the complexity of reasoning in its most abstruse flights. Moreover, in the distinction between the "figure" and the "ground," interpreted in terms of physiological relativity (of cerebral action currents[29]) and generalized to include conceptual processes, we have the beginnings of the doctrine of the movement of thought through the union of opposites into higher intellectual syntheses. As a result of the interaction between an organism and its environment, and between the different systems (gestalten) within the organism, a set of relations appears which cannot be explained in terms of either pole of the duality taken by itself. Thus we are introduced to the concept of *transcendance*, which is necessary to complete the system of gestalt psychology.

According to this view, each whole is a relative whole, being always a pattern within a larger whole, actual or possible. In our thinking we always reach out to larger generalizations, which constitute the system within which propositions secure their meanings. In this respect reasoning is the cerebral counterpart of the general biological differentiation of part patterns within larger wholes (as G. E. Coghill's work shows), while evolution, on the other hand, is the phylogenetic counterpart of the self-transcendance evident in thinking as it moves on to new integrations of activity. In mental life, as in biological evolution and individual development, analysis (differentiation) and synthesis (integration) are complementary and mutually implicative. As the writer has stated elsewhere, the organism and the external world as we know it have developed together by a give and take, in which conceptual interpretation, supplementing perceptual experience, has partially created the forms of externality; and, in turn, the objective order has forced us to readjust our ideas (cerebral behavior) to the semi-determinate texture of the cosmic environment. Through the relatively vast span of time in which this inter-adaptation of organisms and their environments

[29] For a more extended statement of these ideas see the following papers: "The Logic of Gestalt Psychology," *Psych. Rev.*, 1931, Vol. 38, 359–368; and "Biological Relativity," *J. of Philos.*, 1931, Vol. 28, 701–715.

has progressed, the environment has continued to take on a more articulate character, while organisms have continued to develop an apperceptive synthesis to be superimposed upon the external world. Whether this process implies the truth of Dr. Whitehead's[30] speculation that the passage of nature and the passage of mind, which is given in sense-awareness, arises from their both sharing in some ultimate passage which dominates all being is an open question.

Thus far we have not considered such matters as may be classed under the term "abnormalities" of time perception. But the foregoing conception of interaction between organisms and their environments seems to contain the clue to their explanation. Time as we now understand the term in science is to some extent a social concept. That is to say, while it rests upon a correlation of the subjective span of attention with various cosmic rhythms (the rising and setting of the sun, lunar periodicity, seasons, etc.), the measurement of these rhythms and their accurate subdivision into convenient units through the development of chronometers, etc., has come about with the growth of science as a social phenomenon.[31]

Since the objective rhythms "set" the master clock of the human body (the brain), failure to take note of them may result in a disturbance of the subjective time-sense. Patients who are suffering from psychoses may therefore give their age as that at which they entered the asylum, this because they have lost touch with the socially accepted tempos of objective rhythms. In some cases this failure of synchronization is temporary, as for example in the use of drugs. In hashish-intoxication, as James tells us, there is an apparent increase in time-perception. We utter a sentence, and before the end is reached the beginning seems already to date from indefinitely long ago. In his *Confessions of an English Opium Eater* Thomas De Quincey tells of similar

[30] *The Concept of Nature*, p. 69.

[31] For a discussion of the social nature of time see J. T. Shotwell's paper, "The Discovery of Time," *J. of Philos.*, 1915, Vol. 12, pp. 199, 253, 309, ff; the same fact is brought out by Mary Sturt in her book on *The Psychology of Time* and by G. H. Mead's *The Philosophy of the Present.*

distortions of the space and time coördinates. In these cases there is, in addition to the loss of touch with objective rhythms, an alteration of cerebral metabolism. However, different drugs act in different ways: it is argued by Bancroft that some tend to coagulate the colloids of the brain into gels and others tend to liquefy them into sols. It would be an interesting program to study the changes in the time sense as a function of the chemical effects of drugs on the protoplasms.

The Mystery of Scientific Discovery

BY

A. CORNELIUS BENJAMIN

THE extent to which the scientific method has yielded to analysis in recent years serves only to emphasize by contrast the presence within that method of an irrational element. For it is becoming increasingly evident that whatever one may say of the logical and psychological character of the pre-inductive operations, of the formal processes involved in deducing the consequences of a given theory, of the technique of experimental corroboration, and of certain other aspects of the scientific method, there is one phase of the movement which remains a surd. This is the act of the creative imagination through which hypotheses and theories are called into being. Its mysterious character is exhibited in the terminology which is employed in describing it, in the variety and inconstancy of the conditions which give rise to it, in the apparent impossibility of controlling it, and in the impossibility of recognizing completely the adequacy of its results when it does occur. It is variously described as an act of intuition, mystic revelation, divine insight, a flash of understanding; it arises out of the greatest variety of conditions: acute and prolonged attention to the data in the confinement of the laboratory, or the complete relaxation of a walk in the country, or the intense mental effort upon a problem in a wholly unrelated field; it forbids direction, in fact it seems to arise most readily when the element of effort and control is con-

spicuously absent; it cannot even be certainly identified, for many supposed acts of discovery prove later to have produced only groundless fancies and absurd creations of the unguided imagination.

In what follows I shall not attempt to demonstrate anything. I shall try, however, to suggest certain considerations which bear directly on the act of scientific discovery. It will be the aim of the discussion to show that there is more of a formal character in the act than is generally acknowledged.

Unfortunately the elucidation of this proposition demands some preliminary work of a terminological character. I shall make this preparatory material as brief and concise as possible.

The act of scientific discovery is clearly a *movement* of thought. Thinking, in fact, is something that goes on in time; it is a series of acts of awareness of differing contents. Let us use the term, operation, to designate the act by which awareness passes from one content to another. Thinking, then, is a series of operations. In every operation three elements can be distinguished; there is the presented content, the act of transition, and the resultant content. The most common operations are analyzing, inferring, classifying, ordering, numbering, measuring, correlating, substituting, etc.

It is clear, in the second place, that whenever an entity is obtained by means of an operation there must exist a relation of a certain character between the given content and the resultant content. If one infers q from p then p must imply q; if one classifies x's to obtain a class, C, then the various x's must be members of C; if one obtains 5 by adding 2 to 3, then 3 must be less than 5 by 2, etc. In other words the operation is expressive of a relation, and the relation determines a relational-complex. By this I mean that whenever we have two elements in a certain relationship, the character of each of the elements is determined by the nature of the relationship; though this determination may not be specific, it at least excludes certain possibilities. But all negation is determination, hence one may say from an operational point of view that the character of the resultant entity is determined by the character of the given entity and the character of the rela-

tion. This determination will be definite just to the extent to which the relation is definite rather than indefinite. This means simply that if entities are structurally related to one another and if an individual can become aware of the character of that relationship, then there is determined an operation by means of which given one of the entities the other may be obtained for awareness, and that which is obtained for awareness must have a character determined by the nature of the given entity and the relationship. If the resultant entity is given a character incompatible with that which is determined by the nature of the relational-complex, then the relational-complex will be internally inconsistent. This is what is usually meant when we say that the operation has been incorrectly performed.

Hence one may introduce the notion of a *rule*. A rule states what operation is performed and what must be the character of the resultant entity when such an operation is performed. The rule may be said to justify the resultant content in the sense that it enables us to see how it was obtained from the given entity and the operation.

But, in the third place, one must not neglect an obvious fact. Many operations occur without regard for rules. In other words awareness often passes from one content to another without there being any associated awareness of the nature of the transition. The essential character of such operations is that the secondary entity seems to arise out of but does not seem to follow from the presented entity. It merely seems to *occur*, and frequently we see its connection with the presented entity only after it has occurred and been given a content. Let us recognize this difference by calling an operation which is known to take place in accordance with a rule a *derivation*, and an operation which is not known to take place in accordance with a rule a *discovery*. There is no sharp line of demarkation between them, for an investigator may be more or less clearly aware of the rule guiding his operation. It would ordinarily be said, I presume, that from an examination of a quadratic equation one *derives* the solution, but from an examination of heat one *discovers* molecular motion. This difference would be expressible in the terminology given

above by saying that there is a known rule for obtaining the solution of a quadratic equation but no known rule for obtaining the explanation of heat.

One more point, and our preliminary discussion will be completed. I shall use the term, induction, to designate the act of scientific discovery. The term has been used historically to characterize a number of logically distinct processes; I cannot go into these distinctions here. But one of these uses applies to the movement from data to explanatory notion, from fact to theory. It is this connotation which I shall employ in the following discussion. Induction may be defined, then, as that operation by means of which we pass from an awareness of a fact to the awareness of a theory which is explanatory of that fact. But by "explanation" we mean that the fact is such that it could be deduced from the theory. Thus induction and deduction are strictly the converse of one another, and both are operations based upon the structural relation of implication. Thus if p implies q, then given p, we may by an operation deduce q, but given q we may by another operation different in kind induce p. Deduction, then, is inference from premise to conclusion; induction is inference from conclusion to premise.

To show, then, that there is more of a formal character in the act of scientific discovery than is generally recognized, it will be necessary to show that many inductive operations are based on rules. To say that an act is formal, is to say that it is based on a rule and that there is, therefore, a right and a wrong way of performing it. It is clear that deductive operations are based upon rules, hence they are usually called derivations; inductive operations, however, do not seem to be based upon rules, hence they are usually called discoveries. But it is equally clear that many deductive operations can be considered as discoveries to the extent to which we are not clearly aware of the rules justifying them. Oftentimes a deductive conclusion is hit upon in the same spontaneous way as an inductive creation; such a conclusion loses its novel character as soon as we become aware of its connection with the premise and are thus able to look upon it as being contained, in a sense, in the premise. Hence there are genuine cases

of deductive discovery. Are there not also genuine cases of inductive derivation? Are not theories said to be discovered rather than derived merely because we make no effort to determine the rule justifying the operation which would enable us to derive them from the data? I shall attempt to raise certain considerations justifying an affirmative answer to these questions.

But a word of caution is necessary. One must not confuse the psychological and the formal. In searching for such rules one must not be misled into supposing that the causes of the act of discovery are the rules according to which the discovered notion is given content. For example, in preparing for an act of discovery it is frequently advisable to engage in complete mental relaxation, or to divert the mind with wholly unrelated activities, and it is always necessary to desire very eagerly the conclusion which is sought.[1] But these are simply psychological conditions fruitful for the occurrence of the act and do not enable us in any way to determine the content of the discovered notion. It is true that the awareness of the content of the presented entity is both a psychological cause of the occurrence of the act and a foundation for the rule determining the content of the derived entity. I shall not be concerned in what follows with psychological causes. I shall be concerned, rather, with what there is in the character of the data that enables us to obtain from them explanatory notions of a relatively determinate character by means of operations partaking more of the nature of derivations than of discoveries.

(1) The first consideration which occurs to one in support of the thesis expressed above is the obvious fact that the investigator who makes discoveries is always the one who has a firm grasp on the data of the problem. Though we cannot say that familiarity with the facts is the sufficient condition for devising theories, we can say that it is the necessary condition; no one who has not this familiarity can be expected to contribute to the theoretical integration of knowledge. Great inventions by the untutored are extremely rare. This seems to argue for the contention that theories are read out of the data—that they were there all the time, so to speak, but discernible only by prolonged

[1] Montmasson, in his *Invention and the Unconscious*, has emphasized this point.

attention. It certainly argues against the claim, so often made, that theories are discovered by turning away from the facts and toward a metaphysical realm of imaginative entities. The imaginative entities are rather the realm of the real operated upon by precisely those activities which are called induction. It is because the theory has been derived from the data that it is capable of explaining them.

(2) Every act of scientific discovery is followed by a clear and distinct awareness of the relevance of the discovered notion to the data. The cry, "Eureka, I have found it," is a necessary accompaniment of every act of discovery. The theory must have a bearing upon the problem before it can be considered as a discovery. But by the relevance and bearing of an hypothesis is meant its essential relatedness and potential explaining capacity. A recognition of its explanatory value implies a recognition of its status as a premise for the given proposition. And if it is thus definitely related to the given proposition there is presumably a rule which would enable us to operate upon the given proposition to derive it.

(3) In every inductive act the nature of the discovered notion is determined both by one's view as to what constitutes explanation in general, and by one's view as to the specific kind of problem which is presented for solution. If we are searching for identities, as Meyerson maintains, then we do not look for mere rules of action. If we are searching for formal causes we do not look for efficient causes, and if we are searching for causes we do not look for effects. If we are searching for microscopic formulae we do not look for macroscopic, and if we are searching for hidden functional relationships we do not look for qualitative correlations. Thus the discovered notion is not only relevant; it is relevant in a specific way. This means that the hypothetical notion is determinately rather than indeterminately related to the given proposition or its elements. Hence the rule for its derivation must be correspondingly specific.

(4) The leap of the scientific imagination is continuous with and of the same kind as the pre-inductive acts. Every act of discovery presupposes a period of incubation; in Tyndal's phrase,

the act of the imagination is an act of the *prepared* imagination. But what are the factors involved in this preparation? They are operations of essentially the same kind as those involved in the inductive act itself, e.g. analysis, classification, ordering, numbering, measuring, etc. These are operations in the strict sense of the word. Thus the inductive act is continuous with the acts of preparation. In fact the discovered notion is simply a proposition which asserts a particularly significant classification, or an especially fruitful arrangement, or a convenient and fortunate choice of unit in determining measured value. Hence one may say that the explanatory hypothesis is read out of the data in the same way that the data are themselves identified and described. To this extent the Baconian theory of induction seems to be correct.

(5) Closely related to the consideration just mentioned is another which may be expressed in the form of a question: Does not the very attempt to understand the most common inductive operations (abstraction, analogy, generalization) presuppose the conviction that these processes can be formalized? Even more specifically, does not the very success which has been achieved by philosophy in the understanding of these processes offer convincing proof of the fact that they can be formalized? It is impossible to substantiate the affirmative answer to these questions at this time by more than a few random suggestions. For example, the operation of abstraction, which for a long time was looked upon as a vague operation by which one somehow extracted from a number of particulars a certain identity of content is now capable of being formulated, according to Peano, Russell, Rougier and others, as a principle, which enables us when we have given a group of elements between which there is a symmetrical, transitive, reflexive relationship to invent a further element to which each of the given elements then possess a new relationship having certain definite properties. The way in which the method may be used in physics is well exemplified by Lenzen in his *Physical Theory*. Whitehead's principle of extensive abstraction is a further elaboration of the same method and enables us to derive mathematical points by certain logical operations upon any given

extensive quantities which enclose others. Again, the recognition that the validity of analogical inference is dependent upon the nature of the relation between the given qualities and the inferred qualities, permits us to dispense with the particulars and to derive the inferred qualities from the given qualities by virtue of the relation. The principle of mathematical induction has been shown to be a kind of operation by which from data possessing certain definite and specific properties one may derive a universal proposition. In fact the operation of generalization, as a process of deriving universal correlations from a limited number of specific cases, is itself a derivation whose justification demands the making explicit of certain implicit premises which are usually not recognized as contributing to the result. Some authorities would even reduce induction to deduction.[2] All of these operations, therefore, are justifiable by rules provided one takes into consideration all those features of the given proposition which are usually only vaguely recognized. When these features are brought into clear consciousness they are seen to be conditions which determine the induced proposition as a result in a manner which is at least analogous to the way in which a conclusion may be deduced from premises.

(6) The existence of inductive rules would seem to follow from the idea, called to our attention by C. I. Lewis,[3] that the meaning of a concept is simply its structural relations to other concepts. Thus a concept has meaning only as part of a system, and its meaning is clearly or vaguely grasped in proportion as we are clearly or vaguely conscious of its relations to other concepts. A concept is what it implies, what implies it, what is compatible and what is incompatible with it, what is quantitatively related to it, etc. Consequently when we become aware of any concept we inevitably become aware of many other concepts. But each such act of awareness is the awareness of a proposition interrelating the concepts. And in becoming aware of additional concepts we become aware of their structural contents which are in turn propositions. Thus the awareness of the meaning of a

[2] Cohen, *Reason and Nature*, p. 116; Cook Wilson, *Statement and Inference*, p. 585.
[3] *Mind and the World Order*, p. 82.

concept involves implicitly the awareness of the system of propositions about this concept and all related concepts. But such a system of propositions must include all of those which are premises for the given proposition; hence we may be led to these propositions by merely following out the systemic relations.

(7) The next consideration is based upon the notion of operational definitions, emphasized by Bridgman.[4] According to this theory whenever a novel concept is introduced into any science it should be defined in terms of operations, for a concept is "nothing more than a set of operations; the concept is synonymous with the set of operations."[5] Unfortunately Bridgman has said very little about such operations when they are not concerned with measurement. But it is obvious that the theory may be generalized so as to include non-quantitative notions. In fact since definition is itself an operation, it follows necessarily that every definition should be based on an operation. For definition, as an operation, is simply an act of thought by which one passes from the awareness of the concept to be defined to an awareness of better known concepts related to it, plus an awareness of the structural relation uniting the former with the latter. The operation of inventing a novel concept by definition is the converse of the operation of making clear the content of a known concept by definition. Bridgman is evidently trying to insist that a novel concept should always be defined by known operations upon concepts which are themselves directly exemplified in experience; thus every concept will have known relations to the data, and if two concepts have different relations to the same data, or the same relations to different data, or different relations to different data, they should then be considered as having correspondingly different meanings. But this is simply to argue that since every concept has its meaning defined by specific relations to the data, it may be derived from the data by specific operations based upon these relations. Thus the inductive movement is a movement of invention by virtue of which novel concepts are formed through definition in terms of the data.

[4] *Logic of Modern Physics.*
[5] *Logic of Modern Physics*, p. 5.

(8) A further consideration has reference to the general theory of constructs and fictions, which is playing an increasingly important part in modern philosophy. It is an undeniable fact that some of the most valuable of our explanatory notions are concepts which not only are not exemplified in the concrete occurrents of the real world, but in most cases could not be so exemplified, e.g. perfect levers, perfect gases, frictionless motion, isolated individuals, economic cities, mathematical points, lines, etc. The problem with regard to fictions is to explain how things which cannot exist enable us to understand things which do exist. And the solution of the problem depends upon a theory as to the way in which the content of the fictional concept is determined, i.e. a theory of the nature of fictional definition. Now it seems clear that a fiction has explanatory value just to the extent to which it retains in its content certain positive structural relations to the world of real occurrents from which it was derived. In other words every fiction is, as Eddington so well expresses it, a case of "just like this only more so."[6] A fiction would have no explanatory value if it were totally novel; it does explain the realm of real occurrents because it retains in its content, in spite of novel additions and deliberate negations, definite relations to the data, which relations enable us to carry over certain aspects of the fiction and apply them to the data. But since the fiction possesses definite relations to the data, it may be considered as part of their structural content and thus may be derived from them by operations based on these relationships. Fictions are tools, and tools are not discovered but made. On the basis of the material available and the projected task a device is constructed. To be sure, a tool once devised often proves to have use other than that for which it was intended, but this does not argue for the impossibility of inventing a tool for a specific purpose. In fact it might argue, on the contrary, for the presence in the mind of the artificer prior to the inventive act of a vague conception of these other uses. In this case there is clearly nothing spontaneous or mysterious about the inductive act; it is an act of construction, guided by a rule, resulting in a novel element whose content is

[6] *Nature of the Physical World*, p. 247.

what it is precisely because of the nature of the operation performed.

Certain objections to the possibility of inductive derivation may be briefly disposed of.

(1) The following claim may be made: In reducing inductive discovery to the formal derivation of explanatory conceptions, based upon structural relations of the explanatory conceptions to the data to be explained, do you not blind yourself to the fact that the ability to make inductive discoveries is unequally and variously distributed among men? Given a group of scientists confronted with the same data, isn't it a fact too stubborn to be neglected that some will make discoveries and others will not? Is it possible to level down the unequal distribution of scientific genius by its reduction to formal rules? In reply I should not claim that the formalization of the inductive process would destroy all scientific individuality; it will always require a genius to determine which of a given group of possible operations should be performed. But it is also true that as in the case of deduction where one's ability to derive conclusions is developed by an increase in the knowledge of the formal principles of reasoning, and one's inability to derive proper conclusions is due in many cases to one's ignorance of the rules of inference, so in induction the performance of the operation of discovery is dependent upon one's acquaintance with the formal principles which guide this discovery.

(2) How is it possible upon the basis of the theory of inductive operations to explain the fact that from a given group of data a great variety of explanatory conceptions may often be derived? Isn't it possible for two investigators, confronted with the same facts, to devise hypotheses of a different nature? The obvious reply is that one can never be sure either that the data are the same in the two cases or that the hypotheses are different. It is seldom that an attempt is ever made to enumerate and list the complete data in any problem. In fact such a listing would be impossible since the facts contributing to the solution are not only the elements given in some present experiment, but a host of memory facts derived from previous experiments, reading, chance

observation, etc. Furthermore if two hypotheses differ in spite of the fact that they are derived from approximately the same facts and enable one to explain approximately the same group of future facts, it must be due to some underlying resemblance. Indeed it seems necessary to conclude that the element common to the two hypotheses must be precisely that logical factor which gives them their explanatory value, while the differing element must be a psychological factor which is an accidental association and contributes nothing to the explanatory value of the concept. Progress in science consists not so much in the complete abandonment of outworn theories as in the modification of such theories so as to retain their positive values. The presence of this permanent element in all succeeding theories would seem to argue for an underlying structural relationship between the theory and the range of the facts. Hence one need only find the operation expressive of this relationship in order to derive directly, once and for all, the explanatory conception from the facts.

(3) If there are inductive operations which are justifiable by rules, how is it possible to account for the fact that many of the inductive guesses which seem at their inception to be pregnant with possibilities prove later to have no explanatory value, i.e. prove to have been bad guesses? This can be easily answered upon the basis of the obvious analogy to the case of deduction. There are bad deductive thinkers even though there are perfectly precise rules for deductive operations. And there are many apparently correct deductive conclusions, which prove at some later date to be incorrect upon the basis of a more distinct and a more precise conception of the principles of deduction. The presence of rules of inference does not, ipse facto, make us all infallible reasoning machines.

(4) Does not the fact that we are not aware of any rules guiding the inductive operations argue for their non-existence? In deduction the rules have been found and our knowledge of this type of inference has developed correspondingly; but in induction no such rules have been found. If what we have said above is true such rules have been found. But even if they had not this would not argue for their non-existence for the fact of thought antedates its

efficacy. Men thought rationally before Aristotle formulated the rules of thinking, and men will continue to derive correct conclusions without being able to express precisely the rules justifying their operations. The formalization of thinking lags behind thinking itself; thus one cannot hope to complete the formalization of the thinking process. But it appears to be capable of formalization more completely than has been commonly supposed.

DISCUSSION (CORRESPONDENCE)

In this section we shall publish brief communications commenting on published papers or bringing up special points of interest which can be discussed by correspondence.

MEANING, ASSERTION AND PROPOSAL

Sir:

The reading of Dr. Carnap's article "On the Character of Philosophic Problems" has left me with two questions. It is possible that others besides myself would have their attitude toward the general system of Logical Positivism clarified by further discussion of these questions.

1. The view taken as to the relation between formal and connotative (inhaltlich) consideration is not clear to me beyond a certain point. The main thesis maintained is that "it is possible . . . from a viewpoint in which one does not reckon with the meaning, finally to arrive to the answering of all those questions which are formulated as connotative questions" (p. 9). But later on (p. 12) it is stated that "all statements of fact are synthetic"; while on p. 17, it is stated that "mathematical concepts attain their meaning by the fact that the rules of their application in empirical science are given," and on p. 19 the more general statement is made that "what induces us to prefer certain forms of language is the recourse to the empirical material which scientific investigation furnishes."

My question concerns the meaning of connotative or inhaltlich in the contrast drawn; or, putting the question the other way around, what relation, if any, is there between Inhalt and the "empirical material" mentioned in the last two quotations. I get the feeling, possibly quite wrongly, that in the more general thesis as stated 'formal' denotes 'without reference to meaning or Inhalt'; from later portions I derive rather the impression that 'formal' denotes an accurate symbolic statement of Inhalt. ["Fundamentally really there is no difference between the two approaches" (p. 13). And certainly in some of the instances that are given of the two modes of speech, what is given as 'connotative'

is a vague or ambiguous statement even from the side of meaning, while the formal mode is a precise rendering of meaning.]

2. Dr. Carnap distinguishes, clearly and justly, between propositions that are assertions and those that are proposals. How far can this distinction, which seems basic, be treated as propositional in character and how far is reference to 'assertion' and 'proposal' extra-propositional? I am not implying, even in an indirect manner, that we have here a reference to anything psychological, but I should like more light on the exact relation of assertion and proposal to propositions. Of course, we can make the proposition that such and such a proposition is a proposal, not an assertion. But the question then arises: Is *this* proposition an assertion or a proposal? and so on and on.

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A HOME FOR LOGIC

Sir:

Logic, despite the signal advances which mark its history, particularly in the last ninety years, is a discipline without a home. Contributions to it are scattered in heterogeneous periodicals, some of very limited circulation, and discussions of the problems in logic and the foundations of mathematics are little more than tolerated by a large indifferent majority in the meetings of philosophic, mathematical and scientific bodies in this country. There are few who, despite their interest in the topic, are able either to keep abreast of the important contributions or to get the attention of a large number of those who are interested in their field. Would it not be possible for the Philosophy of Science Association to take under its wing the establishment of a Logical Association with yearly national meetings, and a possible series of International Congresses? I think that would not only remove the above difficulties but would considerably quicken and solidify the work of students in the only philosophic field in which there has been at once an underlying common interest and a definitive progress.

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REVIEWS AND NOTES

Symbolic Logic, by Lewis and Langford. Century Philosophy Series, pp. xii+506.

For more than two milleniums the queen of the sciences had been viewed as a discipline begun and completed by a single man within the span of his own life. As late as the eighteenth century Immanuel Kant says in effect: "While it is indeed remarkable that no one has been able to detect any flaw in the logic of Aristotle, it is still more significant that no one has been able to add an important word to what the Stagirite has said." This vast expanse of time corresponds to what we might term the "first age of fixation." The work of the scholastics and their successors, notably Leibnitz, so far as it concerns logic, had left only ripples on the current of human thought.

In 1846 Sir William Hamilton published the prospectus of *A New Analytic of Logical Forms*, a solvent which was soon to make its power felt. No one has ever denied the immense scope as well as the thoroughness of Hamilton's learning, but nearly all historians have disparaged his critical sense. This criticism none the less inaugurated a development, and for the years that immediately succeed, the ancient science is in a state of flux. Two names, De Morgan and Boole, tell most of the story of this time of flux. It is brought to an end by the labors of Peirce and Schroeder. The few years that follow might be termed the "second age of fixation."

Meanwhile other solvents were being prepared. Frege had published his *Begriffsschrift* in 1879, Peano had begun to issue his *Formulaire* in 1895, Whitehead produced his *Universal Algebra* in 1898, Russell his *Principles* in 1903. Finally, 1910–13, appears the *Principia Mathematica* of Russell and Whitehead.

But this last moment of history was no sooner upon us than it had begun to pass. The virtue of this great work lies less in any systematic advance than in its negative result. Beginning with certain promising possibilities, it left them closed. We doubt if the authors themselves

now harbor any illusions on this point (see preface to 2nd Ed.). The famous Sheffer-Nicod reduction is a *reductio* (if not *ad impossibile*) *ad absurdum* of its schematic result. Let no one underestimate its importance on that account. It proceeds from and is founded on the Achilles' heel of the system itself.

It is to Professor Lewis' credit that his own system is in opposition to *Principia Mathematica* at its weakest point. We have (p. 133),

$$12.81\ p \dashv\prec q \cdot \dashv\prec \cdot \sim(p \sim q)$$

and immediately (p. 134),

"The converse of 12.81 which would be

$$\sim(p \sim q) \cdot \dashv\prec . p \dashv\prec q,$$

cannot be proved from our postulates."

The proposition 12.81 is then fundamental to the system, and, as the reviewer has long believed, fundamental to any system, whereas its converse is untrue. Let us consider how the scheme of things may be developed from this as a start. We hope that Professor Lewis would agree to the steps. Starting with the equivalent,

$$p \dashv\prec \Diamond p$$

wherein $\Diamond p = \sim(p \dashv\prec \sim p)$, we obtain a series of results, which we may exhibit diagrammatically in the following table:

| | | |

These results, which seem to follow from the initial assumption, make us wonder at the proposal (C. 13, p. 497) to consider $\Diamond\Diamond p$ as an axiom. It would also exhibit as false some of Becker's assumptions, for example, C.11

$$\Diamond p \cdot \prec \cdot \sim\Diamond\sim\Diamond p$$
$$\Diamond p \cdot = \cdot \sim\Diamond\sim\Diamond p$$

and Professor Lewis' alternative,
C.10.1

$$\Diamond\Diamond p \cdot \prec \cdot \Diamond p$$
$$\Diamond\Diamond p \cdot = \cdot \Diamond p$$

The reviewer would imagine that this last would render the system indistinguishable from the material implication of *Principia* and so invalidate the whole scheme, for,

$$\Diamond\Diamond p \cdot \prec \cdot \Diamond p : \prec : \Diamond p \cdot \prec \cdot p$$

if this operation were permissible, and

$$\sim(p \prec \circ) \cdot \prec \cdot p$$
$$\sim p \cdot \prec \cdot (p \prec \circ)$$
$$\sim(p \sim q) \cdot \prec \cdot (p \sim q \prec \circ)$$
$$\sim(p \sim q) \cdot \prec \cdot (p \prec q),$$

the proposition denied on page 134 and elsewhere. We are of course aware that Prof. Lewis would not allow all these steps.

The other day one of my students, Mr. W. B. Tollen, challenged me to prove Becker's "Brouwer" axiom. The attempt led to certain interesting results, viz. the very inconsistencies of Becker and Lewis. Thus,

$p \prec \sim\Diamond\sim\Diamond p$ gives $\sim\Diamond p \prec \sim\Diamond\sim\Diamond\sim\Diamond p$ $(p = \sim\Diamond p)$
gives $\sim\Diamond\Diamond p \prec \sim\Diamond\sim\Diamond\sim\Diamond\Diamond p$ $(p = \Diamond p)$
gives $\Diamond\sim\Diamond\sim\Diamond\Diamond p \prec \Diamond\Diamond p$

$p \prec \sim\Diamond\sim\Diamond p$ gives $\Diamond p \prec \Diamond\sim\Diamond\sim\Diamond p$
gives $\Diamond\Diamond p \prec \Diamond\sim\Diamond\sim\Diamond\Diamond p$ $(p = \Diamond p)$

$$\therefore \Diamond\Diamond p = \Diamond\sim\Diamond\sim\Diamond\Diamond p$$

A similar substitution was noticed by another student, Mr. Louis Kattsoff, who was working on the same problem.

$\sim\Diamond\sim\Diamond p \prec p$ gives $\sim\Diamond\sim\Diamond\sim\Diamond\Diamond p \prec \sim\Diamond\Diamond p$ $(p = \sim\Diamond\Diamond p)$
gives $\Diamond\Diamond p \prec \Diamond\sim\Diamond\sim\Diamond\Diamond p$

$\sim\Diamond\sim\Diamond p \prec p$ gives $\sim\Diamond\sim\Diamond\Diamond p \prec \Diamond p$ $(p = \Diamond p)$
gives $\Diamond\sim\Diamond\sim\Diamond\Diamond p \prec \Diamond\Diamond p$

$\therefore \Diamond\Diamond p = \Diamond\sim\Diamond\sim\Diamond\Diamond p$

Or, again, if we assume

$$p \prec \sim\Diamond\Diamond\Diamond\sim\Diamond p$$

then $p \prec \sim\Diamond\Diamond\sim\Diamond p$ and $p \prec \sim\Diamond\sim\Diamond p$.

$p \prec \sim\Diamond\sim\Diamond p$ gives $\Diamond\Diamond p \prec \Diamond\Diamond\sim\Diamond p \sim\Diamond p$.
$p \prec \sim\Diamond\Diamond\sim\Diamond p$ gives for $p = \sim\Diamond p$,
$\sim\Diamond p \prec \sim\Diamond\Diamond\sim\Diamond\sim\Diamond p$ gives
$\Diamond\Diamond\sim\Diamond\sim\Diamond p \prec \Diamond p$.

$$\therefore \Diamond\Diamond p \prec \Diamond p$$

Evidently this is a difficulty which cannot be patched up in any ordinary way. The fault might lie in the definition of the modal function and, if we are to retain the "Brouwer" axiom, this function might be redefined. All the properties expressed in our table can be retained if we introduce the following,

Definition: $\Diamond p = \sim(p \prec o) + p$

and a principle of induction, viz.

Principle: $(p \prec \sim\Diamond_n\sim\Diamond p) \prec (p \prec \sim\Diamond_{n+1}\sim\Diamond p)$

where $\sim\Diamond_n\sim\Diamond = \sim\sim\Diamond$, $\sim\Diamond\sim\Diamond$, $\sim\Diamond\Diamond\sim\Diamond$, $\sim\Diamond\Diamond\Diamond\sim\Diamond$, etc. $n = 0, 1, 2, \cdots$

and from this follows the

Theorem: $p \prec \sim\Diamond_n\sim\Diamond p$

since by definition $p \prec \Diamond p$, that is since it is true for $n = 0$. This theorem would be the generalized analogue of the "Brouwer" axiom, in which, however, the meaning of $\Diamond p$ has changed.

The important point to notice is that we can no longer assume,

$$p \prec q \cdot \prec \cdot \Diamond p \prec \Diamond q,$$

which we had to assume to realize our inconsistencies.

An alternative, which has the advantage of enabling us to express a function of any form containing symbols of implication in terms of modals, is to retain the old definition of the modal function and deduce the new by means of an axiom. Thus:

$$\text{Definition: } \Diamond p = \overline{p \prec o}$$
$$\text{Postulate: } p < p \prec o$$
$$\text{Theorem: } p \cdot (p \prec o) < o$$

$$\begin{aligned}
\Diamond p &= \overline{p \prec o} + p \cdot \underline{(p \prec o)} = (p' + p) \cdot \overline{p \prec o} + p \cdot (p \prec o) \\
&= p' \cdot \underline{p \prec o} + p \cdot p \prec o + p \cdot (p \prec o) \\
&= p' \cdot \underline{p \prec o} + p \\
&= p' \cdot p \prec o + \underline{p} + p \cdot \overline{p \prec o} \\
&= \underline{(p' + p)} \cdot p \prec o + p \\
&= p \prec o + p
\end{aligned}$$

In fact for any propositional function, whose form is specific in advance, it will be essential to establish by postulate and theorem that the converse of the propositions exhibited in our table are untrue. If $p(a, b, \cdots)$ is a function of the terms $a, b, \cdots$ this may always be effected by means of the following postulate and principle:

Postulate: There exist certain terms $a, b, \cdots$ having a range of values, such that

$$p(a, b, \cdots) \Diamond \sim p(a, b, \cdots)$$
$$\sim p(a, b, \cdots) \Diamond p(a, b, \cdots)$$

do not vanish.

Suppose the case of the product of two modal functions involving the same parameter, the one factor containing an even number of the symbols ($\sim$), the other an odd number, and the one containing the modal operator more times than the other, and suppose that the operation $\sim\Diamond\sim$ be performed on the factor of higher order. The resulting function may be called the *adjacent* of the factor of higher order, and the same factor negated may be called the adjacent of the factor of lower order.

Principle: If the product of two modals does not vanish, then neither

will the product obtained by replacing either factor by its adjacent vanish.

By means of these assumptions the essential distinctions are preserved for any given case, and of course to found one is to found more than one.

So far we have been engaged with the consideration of a single critical point, but it is the one on which all the others depend. The system cannot be developed much further without the introduction of a new assumption. We require to express any modal function of a product of independent variables in terms of a sum of products of modal functions of the single variables. This is given by,

$$\Diamond(pq) = pq + \sim p \sim q \Diamond p \Diamond q + \sim \Diamond \sim p \Diamond q + \sim \Diamond \sim q \Diamond p$$

which can be readily generalized for any number of variables and for a modal function of any form. Its contradictory follows from the results of our table by the ordinary laws of the algebra,

$$\sim \Diamond(pq) = \sim \Diamond p + \sim \Diamond q + p \sim q \Diamond \sim p + q \sim p \Diamond \sim q$$

These formulas if applied to the theorem announced by Dr. Wajsberg and Dr. Parry (p. 496) will confirm, if we are not mistaken, Professor Lewis' doubt of its validity, which he expresses on the same page. We have not space to develope here the special conditions under which the formulas fail.

If we change q into qr in the first formula, then by easy steps and on reference to the original table,

$$\begin{aligned} \Diamond(pqr) = pqr &+ \sim p \sim q \sim r \Diamond p \Diamond q \Diamond r \\ + \sim p \sim q \Diamond p \Diamond q \sim \Diamond \sim r &+ \Diamond p \sim \Diamond \sim q \sim \Diamond \sim r \\ + \sim p \sim r \Diamond p \Diamond r \sim \Diamond \sim q &+ \Diamond q \sim \Diamond \sim p \sim \Diamond \sim r \\ + \sim q \sim r \Diamond q \Diamond r \sim \Diamond \sim p &+ \Diamond r \sim \Diamond \sim p \sim \Diamond \sim q \end{aligned}$$

and applying the modal operator to both sides of the original equations,

$$\begin{aligned} \Diamond\Diamond(pq) = \Diamond pq &+ \sim \Diamond p \sim \Diamond q \Diamond \Diamond p \Diamond \Diamond q \\ + p \Diamond \sim p \sim \Diamond q \Diamond \Diamond q &+ q \Diamond \sim q \sim \Diamond p \Diamond \Diamond p \\ + \Diamond \sim \Diamond \sim p \Diamond \sim \Diamond q &+ \Diamond \sim \Diamond \sim q \Diamond \sim \Diamond p \\ + \Diamond \Diamond p \sim \Diamond \Diamond \sim q &+ \Diamond \Diamond q \sim \Diamond \Diamond \sim p \end{aligned}$$

$$\begin{aligned} \Diamond \sim \Diamond(pq) = \sim \Diamond pq &+ \Diamond \sim \Diamond p + \Diamond \sim \Diamond q \\ + p \Diamond \sim p \sim \Diamond \sim q \Diamond \Diamond \sim q &+ q \Diamond \sim q \sim \Diamond \sim p \Diamond \Diamond \sim p \end{aligned}$$

The method we have just described is not sufficient to enable us to extend the table of the modal function to infinity in both directions. What *is* necessary we shall set down briefly and without development.

Postulate: $p(a, b, \cdots)$ is not a solution of the equation,

$$\Diamond q \cdot \Diamond \sim q = 0$$

(Since this equation has two unique roots, 0, 1, this is the same as to make of p a variable).

Theorem: $\Diamond p \cdot \Diamond \sim p \cdot < \cdot \Diamond (p \cdot \Diamond \sim p)$
$\Diamond p \cdot \Diamond \sim p \cdot < \cdot \Diamond (\sim p \cdot \Diamond p)$

(These follow by our first expansion formula given below).

Theorem: p is not a solution of the equations,

$$p \cdot \Diamond \sim p = 0, \sim p \cdot \Diamond p = 0.$$

Principle: $\Diamond_{n+2} p < \Diamond_{n+1} p \cdot < \cdot \Diamond_{n+1} p < \Diamond_n p$

Theorem: p is not a solution of the equations,

$$\sim \Diamond_n p \cdot \Diamond_{n+1} p = 0, \sim \Diamond_n \sim p \cdot \Diamond_{n+1} \sim p = 0$$

Definition: A modal function containing an even number of primes ($\sim$) is termed a mode of *affirmation*; otherwise a mode of *negation*.

Definition: Lp, Mp, Np, etc. stand for some modal of p.

Principle: If Mp and Np are the one affirmative, the other negative, and if M is of higher order than N, and if p is not a solution of the equation $M \cdot N = 0$, then p will be a solution of neither of the equations, $\sim \Diamond \sim M \cdot N = 0$, $M \cdot \Diamond \sim M = 0$.

So far we have been concerned only with Professor Lewis' work. We wish we might have space to comment on Professor Langford's skillful exposition and originality of thought, more particularly in his handling of insolubilia. For the reviewer almost all of these difficulties turn on the equivocal use of "all" in the collective and distributive sense. The best example is the paradox of "this proposition is false," which seems to force us to identify this statement with its own contradictory.

If we refer to our original table, the modal functions on the right hand side, since each one contains the symbol ($\sim$) an odd number of times, will be so many and quite distinct ways of asserting the untruth of p. Let us represent any such modal function by ϕ and "p is false" by $f(p)$, so that $\phi(p) = f(p)$. Then the meaning of "false" in any given instance will be the unique root of the equation,

$$f \cdot \sim \phi + \sim f \cdot \phi = 0$$

This is best seen by regarding f and ϕ as independent variables and, keeping ϕ fixed, at one specific meaning, allowing f to pass through its whole range of values.

The other part of the statement, "this proposition," we define as the roots of,

$$\Diamond f \cdot \Diamond \sim f = 0$$

which are easily seen to be unique and to be zero and one, so that "this proposition" is a two-valued function and equal to "this proposition is false" or to "this proposition is false is false" etc. for the two values taken collectively but not for the two values taken distributively. It is interesting to note that as we substitute in succession for p any $f(p)$, the definition simply repeats itself.

We began our review by placing the new logic in its historic place. If we are not mistaken it will occupy that place worthily and perpetuate its influence for a long time to come.

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The Heroic Age of Science. The Conception, Ideals, and Methods of Science among the ancient Greeks, by William Arthur Heidel. Baltimore: The Williams & Wilkins Company. 1933. vii + 203 pp. $2.50.

This is the first volume of a highly promising series of contributions to the history of Greek science which Professor Heidel is preparing. In accordance with its subtitle the book falls into two sections: a long introductory essay on the "Conception and Ideals of Science among the Greeks," and five shorter chapters on "Methods."

The topic of the introductory essay requires much subtler handling than one might expect, and I think Professor Heidel would be the last to claim that his sketch is final. He clears the ground with the pertinent remark: "That the Greeks developed science can be denied only if one defines the name in terms which are applicable to nothing but the most recent formulas . . . the following discussion is intended to show how the Greeks understood science." (p. 1) It should be mentioned that Heidel himself seems to understand by "science" what Whewell and Mill understood by it; but as this is virtually what most writers on scientific method in text-books on logic still have in mind, we presumably have little right to cavil. Be that as it may, the important *obiter dictum* which follows is so acutely phrased as largely to preclude controversy:

"It happens to be my own opinion . . . that the earliest Greek "philosophers" did not in any way trench upon the private domain of philosophy." (p. 1) We learn later that "in a real sense philosophy, as distinguished from science," begins with Socrates, and that "Even when science shared the hospitality of the philosophical schools and perhaps was combined with philosophy in the same person, the two were distinct." (p. 34)

After showing that the early cosmogonies not only were forerunners of science and philosophy but were even recognized as such by Aristotle and Theophrastus, Heidel discusses the rise of the distinction between *mythos*, *logos*, and the *historia* or "research" on which the latter is based; and, referring somewhat gingerly to Plato's use of myths, he adds: "The very fact that the Greeks recognized and consciously utilized this distinction may be reckoned among the best evidences that they had attained a clear conception of science." (p. 10)

Though he feels that the Greeks were peculiarly fortunate in escaping the domination of antiquated theological beliefs, Heidel thinks it a mistake to consider Greek science irreligious, and supports his contention with citations not only from the familiar Xenophanes but also from the Hippocratics, Lucian, and Plutarch. He points out "the emergence of a conviction that science is concerned with proximate causes," and remarks that in the Fifth Century the then dominant conception of *physis* (or "Nature") "without dethroning the gods, . . . did tend to eliminate them as proximate causes and by removing them virtually to an extra-mundane sphere to assign them, so far as philosophy dealt with the question, the sole rôle of first cause in the physical world" (p. 21), though in the long run "Nature came to be regarded . . . as a process complete, self-contained, and all-inclusive. The logical consequences of this view . . . inevitably led to a conception of the world which, as Plutarch says, left no place for the divine principle, except as Nature herself, by acquiring the functions formerly assigned to the gods, became in all but name a divine being." Heidel reminds us, however, that this consummation came only "at the end of Greek history." (p. 22)

Heidel is fond of asserting the "unity" of early Greek science. While he concedes that medicine may perhaps be considered a separate branch, he would have us remember that it had close connections not only with philosophy but also with meteorology and geography, and he suggests that it was really the trade in *materia medica* which led to the geographical expedition of Scylax of Caryanda. He admits that monographs on

special subjects were by no means unknown; but in general he feels that the division of labor in the Lyceum which assigned the history of botany and natural philosophy to Theophrastus, that of astronomy and mathematics to Eudemus, and that of medicine to Meno, has resulted in an impression that Greek science was more departmentalized than it actually was, just as he feels that the "predominantly metaphysical" interest of Aristotle and Theophrastus has vitiated the whole tradition as to Greek science.

The remainder of the essay is devoted largely to a useful summary of Greek theories of the "elements" as "causes" or categories of scientific explanation, and to a discussion of "the principle that everything has a natural cause." Curiously enough, Heidel considers the Greek astrology the chief instance of belief in this principle, but he reassures us that in spite of this the Greeks never let themselves be taken in by astrological fatalism. He points out furthermore several cases of scientific wariness where Greek authors have either recognized the limitations of their present range of knowledge, or—as in the Epicurean theory of the primordial atomic swerve—have even taken advantage of them; in this connection he also considers the idea of chaos. The essay closes with other instances of Greek respect for directly observed phenomena, judiciously culled from Thucydides, Aristoxenus, and the Hippocratics.

The second half of the book—"Methods"—opens with an introductory chapter, in which we are reminded that "both Aristotle and modern science go in for deduction; the important difference is that Aristotle's inductive technique is not so good as it ought to be." (p. 68)

It would hardly be fair to summarize the next four chapters on "Observation and Induction," "Classification," "Analogy," and "Experimentation;" for then the reader might be tempted to skip them, and certainly no one who wishes a clear notion of the range and penetration of these methods among the Greeks should leave them unread. They are the most useful part of a valuable book, amply furnished with examples both apposite and unhackneyed. To be sure, the sparseness of the material is sometimes more revealing than its positive content, especially in the first of these chapters; but whether we are to lay the blame for this on the omissions of the post-Aristotelian tradition or on the inadequacy of Greek science itself is unfortunately still for the most part a matter of preference.

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A NEW BUDGET OF PARADOXES

(2) CLASSIFICATION OF PARADOXERS

We have been asked to explain our classification of paradoxers. Consider, therefore,

Class I. Emulsion-Makers
A. Specialists in Non-sequiturs, thumb logic
B. Eclectic Kleptomaniacs

The "non-sequitur" is familiar, but what makes a specialist? A specialist in the art of Non-sequitur is one who can made the blend "A is non-A" palatable. His is, of course, a psychological art, not a logical one.

And thumb logic? That is a technique of seeming systematic doubt, currently abused by Talmudists, a kind of "eductio ex absurdo." Anything may be negated at any point in the discussion where a positive statement emerges. The process of asserting the negation is accompanied by a characteristic swing of the thumb. Now, who can tell what n negatives indiscriminately distributed may lead to? Let us call it an emulsion.

Eclectic kleptomania is the irresistible desire to attach to oneself by surface forces varieties of the quotable,—in itself a harmless and even scholarly proceding, but easily prone to become "shotgun medicine," a cure-all, a milk supercharged with vitamins but devoid of substance.

For illustration, we are reminded of a German work devoted to the explanation of the oddly named philosophy of "Monismusdualismus." One must not imagine for a moment, however, that it is the same as "dualismusmonismus." The latter is, as chemists will readily realize, another kind of emulsion altogether.

"Matter's Thinking," 1713, may or may not be a foolish title. Yet, the emulsion has not settled out these two centuries.

For thumb logic we refer the reader to Phipson's address to the British Association for the Advancement of Science. As late as September

1886 he evokes a "thumbed" conclusion: "The matter of all the elements is therefore identical; the phlogiston alone varies."

Literature, indeed, yields more delicate emulsions. Note Amiel's: "Materialism coarsens and petrifies everything: makes everything vulgar, and every *truth false*." Or, even better, Balzac's: "God is a Number endowed with motion, which is felt but not demonstrated."

Again we invite clinical contributions on choice cases.

W. M. M.

NOTICES

CORRECTIONS TO VOLUME 1, NO. 1

Carnap "Philosophical Problems"

Page 10, line 12 from bottom—instead of "Hilbert's mathematics" read "Hilbert's metamathematics" (likewise line 9 from bottom, but not line 11 from bottom).

Page 12 line 4 from bottom—instead of "obtained from" read "contained in."

Page 7. Citations from Hume, add:

> "All other enquiries of men regard only matter of fact and existence; and these are evidently incapable of demonstration. . . . When we run over libraries, persuaded of these principles, what havoc must we make? If we take in our hand any volume; of divinity or school metaphysics, for instance; let us ask, Does it contain any abstract reasoning concerning quantity or number? No. Does it contain any experimental reasoning concerning matter of fact and existence? No. Commit it then to the flames: for it can contain nothing but sophistry and illusion." (Hume: An Enquiry Concerning Human Understanding, XII, 3.)

FORTHCOMING PAPERS

On the Construction of Functional Classes in a Mechanical Medium, by E. A. Singer, Jr.

Abstract Logic or the Science of Modality, by H. B. Smith.

What is Analysis? by A. E. Blumberg.

An Examination of the Quantum Theories, III, by W. M. Malisoff.

The Psycho-Physical Problem, by H. Feigl.

Philosophy of Science in the U. S. A., by M. R. Cohen.

What is the Case? by L. S. Stebbing.

Relativity and Thermodynamics, by R. C. Tolman.

Critique of Freud, by J. F. Brown.

The Method of Physical Coincidences, by W. Bender.